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STUDIES IN AFFECTIVE PSYCHOLOGY

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I. THE LOCALIZATION AND SPATIAL CHARACTER OF PLEASANTNESS AND UNPLEASANTNESS

The aim of the present study is to throw light upon the following questions: (1) Are pleasantness (*P*) and unpleasantness (*U*) capable of localization in space? (2) Do pleasantness and unpleasantness possess a spatial character of volume, similar, possibly, to the volumic attribute of tones?

Method and Procedure. The experimenter sought to arouse a moderately intensive *P* or *U* in a direct but non-mechanical manner through the presentation of olfactory, gustatory, auditory, or tactual objects. When he judged that *P* or *U* had been aroused in *O*, he pressed a button which gave a rap upon a sounder—the prearranged signal for *O*'s report.[1] Sometimes there were bodily indications that an affective process had been evoked, as an incipient smile, a movement of avoidance or the involuntary twitching of facial muscles. When there was no hint of bodily movement the experimenter waited until he concluded that a feeling had been aroused.

O was seated in a Morris chair, his eyes closed. The chair was in an enclosure, 7×9 ft., curtained from the rest of the room. The stimuli were arranged on tables outside the enclosure. In an actual experiment E took a stimulus-object into the enclosure and stimulated *O*. When he thought that a feeling had been aroused he pressed the button and retired to copy down the report.

Stimulus-objects. The following objects and forms of stimulation were used in the main experiment:

Olfactory: white rose perfume, anise, stale cheese, sweet pea perfume, oil of cloves, heliotrope, lemon, oil of bergamot, H_2S, white locust perfume, camphor, oil of lavender, putrid fish, honey-suckle perfume, oil of juniper, nutmeg.

Gustatory: candy, citric acid, sarsaparilla syrup, quinine.

Auditory: crumpling paper near ear, friction of sandpaper held near ear, clangs from tuning forks—e g c′ and e g c′ a, noise from clapping pieces of wood together, from filing glass near *O*'s ear, and from falling metal.

Tactual: sandpapering of chin, cotton on cheek, removal of foot rest, tickling nose with camel's hair brush, rapping on knee or forehead with

[1]The present method should be compared with that of Nafe who instructed his *Os* to judge when the feeling was at its maximum and then to break off the observation. In a later experiment, repeating Nafe, one of my *Os* reported difficulty in determining when the feeling was at its maximum while at the same time he observed the feeling. J. P. Nafe, An experimental study of the affective qualities, this JOURNAL, 1924, 35, 510.

small hammer, pressing needle through stocking, snapping rubber band on face, dashing water on face, pinching nose with fingers, slapping face warming with blackened carbon light, holding lighted match near face.

Instructions. The following instructions was used: "Put yourself so far as possible in an attitude of indifference. Odors, sounds, tactual impressions, and tastes will be presented to you from time to time. When you hear the tap of the sound-hammer please make a report. You are instructed to describe the quality and the spatial characteristics of the feeling, noting especially the place where it is located."

At the close of the experiment the following questions were asked: (1) Judging from your observation in this experiment, are *P* and *U* experienced at some place? (2) How do you understand the instruction to describe "the spatial characteristics of the feeling, noting especially the place where it is located?"

The answers which the *O*s gave to the questions are presented below with the data.

Observers. The experiment was conducted during the fall of the year 1922 in the Psychological Laboratory of the University of Illinois. Five *O*s took part in the experiment, all of them connected with the Department; C. R. Griffith (G), G. D. Higginson (H), H. L. Kingsley (K), Alice H. Sullivan (S), Dorothea E. Wickham (W).

No fixed number of observations were taken on any day; the number varied with the length of report. Every *O* made a total of 65 reports during the experiment, except W who made 55. The total number of observations in the experiment is 315.

Summary of Reports

Observer H: There is no case of localized *P* and *U* in the reports of H. Furthermore, there are positive statements to the effect that affective processes are not localized, for example:

"I seem to localize the odor but I don't seem to localize the *P* of it."

"There was no (affective) localization so far as I could see. The sound, of course, was localized."

"Definite localization of cutaneous and kinesthetic qualities, but I could observe no localization of *U* or *P*."

"I did not localize the *P* in any sense, but the relaxation was quite clear."

Sometimes H reports that feelings are general to the entire body, as:

"A *P* feeling seemed to be general to my entire body."

"The feeling seemed to be general through my body. It was not localized."

At the close of the experiment H answered the two questions about localization and the instructions as follows:

(1) "In the light of my observations during this series of experiments, I am quite sure that I can state with emphasis that pleasantness and unpleasantness, for me, are not experienced at any particular place with reference to my body." (2) "I understand the instruction 'note the place where the feeling is located' to mean any particular region to which the particular feeling under consideration is referred or found or sensed, as on any general area of the body. 'The spatial characteristics of the feeling' I took to mean the relative spread-out-ness or diffuseness, or the general limits of a feeling as it is observed at or in any particular region or place."

Thus, so far as H is concerned, the evidence for the localization of *P* and *U* is completely negative, and the reports furnish

no data on the spatial character of *P* and *U* except a few statements that these feelings may be general, throughout the body.

Observer S: S reports localized feelings, for example:

"That was very *U* and nauseating and that feeling was localized in my chest and around my stomach. The feeling seemed to be made up of sensations—little pressures and things like that in my stomach."

"The localization of the *P* feeling seemed to be very diffuse. It was all over me and enveloped me. The localization of the *U* feeling was right within me—chest and stomach—and it was small compared to the *P* feeling."

"*U* was located around my mouth somewhere; not in my mouth. It must be a sense-feeling because the *U* is right there with the sweetness."

"A feeling of *P*, widely diffused, which became more widespread. The feeling began somewhere in the region of the chest and spread over the body and outside of me."

"A feeling of *U* which increased in intensity and in extent. It was localized partly in the chest and partly outside of me, and as the stimulus continued the *U* shot back and forth in streaks. It was rough in texture and had no definite boundary line."

"Very intense *U*. It was spread all over the body as if I were immersed in it. The feeling was soft."

"*P* seemed to be localized around the stimulus out in space. It was soft and fluffy."

"Moderate *U* which was localized in the nose."

"Very intense *U* which seemed to be localized in my head."

The reader will have noticed in reading the above quotations that *P* and *U* are sometimes described as extended, surrounding the object, having a voluminous out-spread. The following excerpts from other of her reports illustrate further this spatial character of the affective processes:

"A great expansive feeling which surrounded the stimulus. The expansive feeling seemed to emanate from the stimulus outside of me. It seemed as though the most *U* part was near the stimulus and then it got less and less intensely *U*. I couldn't say where it ended."

"*P* was soft and filmy and much brighter than *U*. It had rays of *P*, more intense, scattered around. In between the rays there was weak *P* and the rays were stronger *P*. It was quite extended but it wasn't in me. It was outside of me surrounding the stimulus. It was as if the stimulus were in the center and then these little rays extended out from it."

"The *U* seems to begin in the mouth and to spread out around. It spreads out in rays. It goes out in one plane. It is a flat thing. Some of it goes outside of me and some of it is in me. These rays seem bits of more intense *U*."

"That was *P* and it was spread out all around the odor. It was grayish with just a little tinge of lavender in it (S has synaesthetic imagery). It seemed to be wrinkled. It didn't have rays. It spread out in a circle with wrinkles of more intense *P*."

If we take the above reports at face value, we may say in general that the affective processes have a spatial attribute. *P* and *U* are extended in a spatial pattern which may have gradations in intensity, the feelings being more intense in the center and less at the periphery. The spatial pattern may have

streaks, rays or wrinkles, differences in texture. The affections are large or small in volume; boundaries are not clearly marked off.

Sometimes the feelings are very voluminous, diffuse, widespread, as in the following examples:

"The feeling seemed to spread out all over me. It was as if I were within the experience and it were all around me. The feeling was diffuse."

"The *P* was like a cloud which enveloped me. It wasn't localized within me; I seemed to be localized within it."

"That was *P*; it was diffuse, large. It seemed to envelop me and was not localized within me."

"*P* seemed to be a cloud-like thing that surrounded me and was outside of me. I couldn't tell where it ended or where it began."

S answered the two questions as follows:

(1) "Pleasantness was, for me, never experienced at any place within me. In most cases I think it is non-localizable; in some cases, however, it seems to be localized around the stimulus. But even in the latter case the localization is not clear, for I can not say where it begins or ends. Unpleasantness was quite often fused with organic sensation and was then localized where the sensation was. I think it is also non-localizable where experienced alone. By non-localizable I mean that I can not locate it in space any more than I can locate it within me." (2) 'I understand the instruction 'to describe the spatial characteristics of the feeling, etc.,' to mean that I am to tell how extended the feeling is and whether or not it has a boundary line, and its pattern. Sometimes I can describe the pattern of a feeling and yet I can not localize it. I understand the instruction about noting 'the place where the feeling is located' to mean that I am to note whether it is localized within the body as an organic or kinesthetic sensation is located or outside the body, around me, in front of me, etc."

If I understand correctly the answer to the first question, *P* and *U*, in themselves considered, are not localized, or possibly they are very vaguely localized around the stimulus-object. S's reports contain a good many statements to the effect that *P* and *U* are localized, but there is some doubt about the place and the fact of localization.[2]

S explains that the local character is due to the sensory process rather than to the affective process. In other words it is a sense-feeling, a fusion of sensory and affective processes, which is localized. The sensory rather than the affective component is local.

Observer G: G sometimes reports localized feelings, as for example:

"Decidedly *P*. The tickle itself was *P*, and the *P* quality seemed to be located in the nostrils and at least about the face."

"The auditory quality was *U*. It was localized there about my ear. It was just a *U* sound; it was that kind of a sound."

"The tactual quality itself was *U*. It was a *U* right there at my forehead."

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[2]There are at least a dozen statements in the 65 reports expressing doubt about the localization: "I don't know where the feeling is." "I cannot say where it is localized." "I am in doubt about the localization."

"Withdrawal and a facial configuration meaning disgust. That taken with organic qualities gave a nausea. The whole experience was *U* in general and in particular the odor was *U* and the organic qualities were *U*. In a certain sense, then, the *U* was localized at the nostrils and in the viscera."

"*U* located with taste qualities on the tongue."

"Auditory qualities increasingly *P*. The *P* feeling was an integral part of the tonal experience, located with the tone, a character which the tone possessed."

"The *U* is part of the odor and located with the odor in the nostrils."

"The *P* was localized along with the tactual qualities and a resident part of them. A more diffuse and obscure feeling of *P*, a feeling of comfort, carried in the total character of the organic and kinesthetic processes, an obscure background of *P* feeling."

P and *U* are sometimes described as diffuse, widespread, voluminous:

"There was a momentary *U*, diffuse and not localized anywhere; a general feeling of *U*."

"An organic and kinesthetic pattern, diffuse *U*."

"Slightly *U*, widespread over the body."

"An organic and kinesthetic pattern, widespread, unlocalized, *U*."

At the close of the experiment G answered the two questions as follows:

(1) "A considerable number of my affective experiences appeared to be very definitely localized. Frequently they were localized along with the perceptual experience itself and less often they appeared to be located along with organic processes. Occasionally the affective experience was unlocalizable but almost always (in this case) the related organic processes were diffuse and obscure." (2) "I understand the instruction to mean the localization of affective experiences in the same sense as other objects are located. The tones from the tuning fork out there are pleasant 'out there.' The organic sensations aroused by a nauseating odor are near or in the diaphragm or stomach or viscera and the affective quality is similarly placed."

At the start of the experiment G did not localize *P* and *U* and there are a number of definite statements to the effect that *P* and *U* are not localized. As the experiment progressed *P* and *U* feelings were localized along with the sensory processes and at the place of the sensory processes. G's answers to the questions confirm the view that *P* and *U* feelings are definitely localized.

Sometimes the localization is not exact; *P* and *U* feelings are diffused over the whole body or in some general region of the body.

The reports show unmistakably that G regards *P* and *U* as characteristics of the sensory processes or of the total experience. *P* and *U* go along with, depend upon, attach to, belong to, are tied up with, are referred to, are characteristics of, some sensory pattern. The sensation *is P* or *U*; it is 'that kind of a thing.'

G is skeptical about the existence of affective processes and speaks of *P* and *U* as meanings carried by sensory processes.

Altogether it appears from G's reports that sensory feelings are localized, and that so far as *P* and *U* are reported to be localized the local character is derived from localized sensory processes. The reports furnish no evidence that the affective processes, *P* and *U*, considered in their own right apart from sensory processes, are localized.

I hazard the guess that the spatial character of diffuseness or volume is also referable to sensory processes.

Observer K: Some examples of localized *P* and *U* by K are given below:

"As far as the *U* was localized it was all through the upper part of my body. Not localized at any point."

"That was a *P* sensation and it seemed to be localized entirely in the nose region."

"That was *P*. In a general way it seemed localized in the region of my head."

"At first the *U* was felt in the region of the sensation, as it seems. Later it seemed to be diffuse and general."

P and *U* may be diffuse and extended, as for example:

"The effect was spread out, not localized. The sensation was spread out and that gave me the impression of the *U* being rather diffuse."

"That was a *P* odor. It was rather spread out, diffuse, without limitations. It seems to me as if the feeling is spread out in the upper part of the nose, in my throat and back of my mouth."

"That was *P*. It was diffused all through the upper part of the body and arms with a more intense form around the nostrils where I got the odor. It was in the upper part of my body as far down as the stomach. It seemed to come to a head being more intense, thicker, in the region of the nostrils and less intense as it spreads through my body. I almost visualize it in this way."

"*P* feeling. It seemed to be sort of a general muscular response, very diffuse."

"I cannot localize the feeling. The experience in general was in the region of the nasal cavity and the upper throat. Attention was directed there and what I observed there seemed to be the agreeable, and pleasure-giving thing."

Some of K's reports indicate an uncertainty about the distinction between sensory and affective processes and consequent difficulty in reporting upon localization and spatial character of *P* and *U*:

"That was a *P* odor and I seemed to feel it part way through my throat. I am confused about the localization; I don't know whether I am to distinguish between feeling and sensation. I feel the sensation; that's a naïve way of saying it. I felt that one as far down as the neck. The agreeable sensation was in the throat. It seemed to be very diffuse and general."

"The stimulus attracts my attention and it seems as though the feeling is concentrated there. With more careful observation I don't think I could say the feeling is located there because of other experiences that come with it."

"That was *U* and the experience was pretty well spread over my body. The muscles of my nose tended to pucker. What I localized seems to be sensory experience."

"That was *P* and I seemed to feel it in the nasal cavity and down the throat and even in the lungs. At least I refer the feelings to these regions. It is very hard for me to say whether that is feeling or sensation. As nearly as I could tell the feeling was there and even outside of the face. I localized it there."

At the close of the experiment K answered the questions as follows:

(1) "Judging from my observations in this experiment, *P* and *U* are experienced at some place—if we are to understand 'place' as referring to rather extensive and indefinite regions of the body. They, *U* and *P*, were not localized as sensations of taste and touch are localized, at some definite point, but more as general bodily kinesthesis is localized, vaguely within some region of the body." (2) "I understand by the instruction 'to describe the spatial characteristics of the feeling, noting where it is located,' that I was to observe its size and shape and location, meaning by location the place within or without the body where it seemed to exist. I understand that I must try to observe whether I localize the feeling as I do sensations, as when I experience pain *in* the end of my finger."

I have quoted above examples of localized feelings. It would be equally possible to quote examples where localization was doubtful or impossible. For illustrative purposes a few examples are given below:

"I don't know whether I can speak of the localization of the *U*. Of course the sensation was clearly localized."

"Moderately *U*. I am unable to say anything about the localization of it." "The *P* which I felt couldn't be localized or described spatially at all."

"I can't say that *P* and *U* are localized, but attention is towards the stimulus and the experience seems to be there."

"*P*; it seemed to me that the stimulus took my attention so that I couldn't make any localization."

If we take K's reports at face value, we may conclude that *P* and *U* are sometimes localized in space and sometimes diffused in a general region of the body, over the entire body, or even outside, enveloping the body.

The main trouble with this conclusion is that it is ambiguous. K is not clear about the distinction between sensory and affective processes. The localized feelings are observed as sensory processes, or as sense-feelings, and it is probable, judging from the reports, that the local character rests directly upon sensory experience. K attends to sensory processes which *are P* (or *U*) and it seems as if the *P* (or *U*) feeling is there.

Observer W: A few quotations from W's reports bearing upon the problems of affective localization and volume are given below:

"A strong feeling of *U*; a desire to draw away from it. The feeling seemed to be largely in the region of the head and shoulders and arms. It was not diffused throughout the body."

"I enjoyed the candy very much. I had the quality of *P* so far as the sensation of taste was concerned. It seemed to be localized in the region of the mouth and throat. I didn't seem to be able to locate the *P* any place other than in my mouth or in my throat."

"I seemed to feel a certain feeling of *P* spreading through me. A general bodily contentment. I should like to have that sensation for some time. The localization probably took a region from the nostrils to my ears."

"A vague *U* which seemed centered around my ear drum."

W's type of report is quite naïve and, as it happens, her data do not throw much light on the experimental problems. Sense-feelings are localized in the region of the sensory processes. The distinction between sensation and affection is not clearly drawn, so nothing can be said definitely about the localization of *P* and *U* as affective processes.

General Summary

If we depend upon statements made by the *O*s at the close of the experiment, we find lack of agreement upon the matter of affective localization. Two *O*s (G and K) state that *P* and *U* were localized; two (H and S) state that *P* and *U* were not localized. One (W) was not given the questions, but nevertheless localized feelings occur in her reports.

I am not willing to infer that this disagreement reflects any genuine difference in the experience of the *O*s—although this is a possibility. It seems rather that the disagreement rests upon different understanding of certain critical words, specifically:

(1) The term 'feeling' is used by the *O*s to indicate (a) clear sensory processes, especially organic and kinesthetic processes, (b) sense-feelings as, for example, an odor which has the character of *P* or which means *U*, or which possibly is a fusion of sensory process, and (c) non-sensory, non-clear affective processes, *P* and *U*, differing in quality, intensity and in temporal course.

When the observers used the term 'feeling' in the sense of (a) or (b), localization in perceptual space was sometimes reported.[3] In the sense of (c), localization was not reported.

(2) The term 'localize' is also ambiguous. This word may signify: (a) to *refer P*, *U*, or other 'feelings' to some place in perceptual space. K, for example, used the term in this sense. Localization meant reference to the sensory object—odor, taste, sound, etc.; or to the bodily processes secondarily evoked by the sensory object—nausea, relaxation, etc. From this point of view all of the *O*s 'localize' feeling. (b) 'Feelings' are said to run their course in a region of the body without very precise spatial localization, or they may be general to perceptual space, diffuse, voluminous. The term 'localize' sometimes means merely the noting of the space where the 'feelings' are given and the description of the spatial characteristics of the 'feeling' ex-

[3]The instruction used the term 'feeling' without defining it, whereas question (1) asked specifically about the localization of *P* and *U*. The *O*s usually took the term 'feeling' to refer to *P* and *U* feelings, but if the terms of the instruction had been precisely defined, possibly the ambiguity would not have occurred.

perience. (c) The term 'localize' may mean to place affective processes within the phenomenal field as components of it in a non-spatial, non-perceptual sense. I have no evidence that this third possibility occurred to any *O*.[4]

In the sense of (b) there is a good deal of evidence that *P* and *U* 'feelings' have the spatial attribute of volume—bigness, diffuseness, outspread. *P* and *U* may be restricted to some gross anatomical region or they may be general, unlimited, everywhere. *P* and *U* may spread out around the sensory object. *P*, as Nafe has noted,[5] is generally more voluminous than *U*. The evidence from the present observations seems to indicate that this voluminous outspread is sensory in nature.

Conclusion

The *O*s disagree upon the matter of localization of *P* and *U*. It is not likely that the differences in the reports reflect genuine differences in affective experience. It is more probable that the disagreement depends upon the ambiguity of certain words, specifically the terms 'feeling' and 'localize.' These terms may be understood in a manner justifying the conclusion that '*P* and *U* are not localizable;' in another manner the conclusion follows that '*P* and *U* are localizable.'

The experiment brings to clear light the question of the existence and nature of the traditional affective processes, and a serious difficulty with the introspective technique.

Relation to Other Experiments

In a previous brief paper upon localization I concluded that "(1) *P* and *U* are not localizable. Localization and extent are characteristics of the unanalysed object-feeling of common-sense. (2) Localization and also qualitative differences of feeling depend upon sensory components in the total feeling experience. The popular term 'feeling' includes localized sensation as well as meaning."[6] Also in criticising Wohlgemuth I have defended this position.[7]

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[4] Cf. E. B. Titchener, *Lectures on the elementary psychology of feeling and attention*, 1908, 43 *f.*

[5] *Op. cit.*, 523.

[6] P. T. Young, The localization of feeling, this Journal, 29, 1918, 430.

[7] P. T. Young, The coexistence and localization of feelings, *Brit. J. Psychol.*, 15, 1925, 356-362.

Wohlgemuth has published a rejoinder which does not seem to advance the problems in question. The interested reader should refer to the original papers in this controversy and then form his own conclusions.

The net outcome of the discussion seems to be the drawing of a distinction between (1) the cognitive awareness of a pleasant or unpleasant object and (2) an immediately felt pleasantness or unpleasantness. This distinction can be drawn on the basis of Wohlgemuth's published introspections, for example: "I seemed at the same time to know that there was a certain quality, a sort of whistling quality, connected with the tone that was disagreeable without feeling it to be so." (A. Wohlgemuth, Pleasure-Unpleasure: An experimental investigation on the feeling-elements. *Brit. J. Psychol., Monog. Suppl.*, 6, 1919, 95.) In the sense of (1) there is ample evidence that two or more pleasant and unpleasant objects may be cognitively apprehended at the same time. If by 'feeling' one mean the awareness of a pleasant or unpleasant object, then 'feelings' are localized and they may coexist. If by 'feeling' one mean pleasantness and unpleasantness reported as felt and as having a temporal course in experience during which qualitative shifts and intensive changes may occur, then the possibility of coexistence and localization of the affective processes has yet to be demonstrated for the evidence is mainly negative.

At the close of his rejoinder Wohlgemuth refers in a foot-note to Phelan's recent monograph upon feeling. I fail to see how this study advances the problems of coexistence and

I believe that the positions I have taken in these studies of localization presuppose a certain systematic position regarding the nature of affective processes—the position which was in the air at Cornell University in 1917-18. Starting from the view that there exist non-clear, non-sensory affective processes which vary in quality (*P* and *U*), in intensity, and in temporal course, the papers I wrote have a certain validity. The validity is relative to the postulates which underlie these studies. Given another set of definitions and psychological postulates the results might be reversed. The answer to the question of localization may be 'yes' or 'no' according to what one understands by 'feeling' and by 'localization.'[8]

The Effects of Practice in Observing. Nafe has written: "Many of the *O*s, in their earlier reports, attempt to localize the affection, and often mention a more or less definite region where it is felt. Later, however, they speak of a 'reference' to a part of the body and deny the localization. All the *O*s finally report affection to be unlocalizable, but they may still refer it to their whole body, and the affection may include the body or be projected out from it."[9] This statement implies that as the experiment progressed and as the *O*s became more practiced in observing, the tendency to report localized feelings disappeared. In other words, practice made them 'better.'

With Wohlgemuth the 'improvement' was in the reverse direction. Wohlgemuth writes: "Subjects P and J are unable to localize the feeling-tone at the beginning of the investigation, but as the training proceeds statements to that effect became more frequent."[10] Also in another study Wohlgemuth concludes that "The ability to localize the feeling-elements improves greatly with practice."[11]

In my present experiment repeated observations under outwardly controlled conditions (up to 65 reports) did not bring the observers into agreement and there is little indication that further practice would bring any unity. Had I educated my *O*s in the precise use of certain critical terms,

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localization of *P* and *U*; but the following quotation is suggestive: "There is no *a priori* reason why we should conclude that an individual *feels* a pleasant or an unpleasant response when he simply states that the stimulus-object is pleasant or unpleasant. Furthermore, there is ample *a posteriori* evidence to show that judgments of value, whether of hedonic or aesthetic value, are clearly differentiated, by the subjects themselves, from the actual experience of affective phenomena. As a matter of fact, we have reports of experiments in which it is explicitly asserted that no feeling was experienced, yet the subjects formulate judgments of value in terms of pleasantness and unpleasantness. On the other hand, there are numerous cases in which the affective character of the experience is actually *felt*, and, in addition, a judgment of value is made, which is, on the subjects' own statements, quite distinct from the feeling itself, and, sometimes, quite contrary to it." G. B. Phelan, *Feeling experience and its modalities*, 1925, 64.

It is essential, therefore, to distinguish between the affective judgment which characterizes an experience as *P* or *U*, and the actual *felt P* or *U* upon which the judgment may or may not depend. Wohlgemuth has not eliminated the affective judgment of objects in reporting his own 'mixed feeling' in which "pleasant daffodils" and "unpleasant foot-sensations" were present simultaneously in consciousness. A. Wohlgemuth, The coexistence and localization of feeling, *Brit. J. Psychol.*, 16, 1925, 116-122.

[8]Titchener has stated his belief that "affections are always coextensive with consciousness, diffused over all the sensory contents present at the time; and that, if the pleasantness of a taste is localized in the mouth, that is simply because consciousness itself, under the experimental conditions, has been narrowed down to a taste-consciousness." E. B. Titchener, *A text-book of psychology*, 1915, 234-235. Cf. also *Lectures on the elementary psychology of feeling and attention*, 1908, 43 f.

[9]*Op. cit.*, 535.

[10]A. Wohlgemuth, On the feelings and their neural correlate, with an examination of the nature of pain, *Brit. J. Psychol.*, 8, 1917, 450.

[11]A. Wohlgemuth, Pleasure-unpleasure, etc., *Brit. J. Psychol., Monog. Suppl.*, 6, 1919, 242.

had I laid down the definition of these terms and the specific task of observation, I might then have found uniformity. It would, however, be a *predetermined* uniformity more than a *discovered* uniformity.

II. The Case for the Affective Processes

The present experiment, dealing with the nature of the affective processes, was conducted immediately after the preceding experiment upon localization.[12] The experimental setting, the stimuli and other details were the same as in the earlier experiment. Three of the *Os* were retained.

Instructions. Throughout the experiment a general instruction was used as follows:

"You will be presented with simple odors, tastes, sounds and tactual impressions. Report when you hear the rap of the sound hammer."

Along with the general instructions were used in turn two specific instructions which, it was hoped, would throw some light upon the nature of the affective processes. They are:

(1) "Describe your feeling experience, mentioning temporal relations precisely. Put direct description of the observable content-processes of experience outside of parentheses.[13] Put all else inside."

(2) "The aim of this experiment is to determine the fundamental nature of feeling. Whenever you use terms which refer to feeling state exactly what these terms mean. Analyse the parts or phases of experience included under the term 'feeling.' "

Summary of reports. The present experiment comprises 153 reports distributed as follows:

Os	Instruction 1	Instruction 2	Total
S	30	32	62
G	30	24	54
K	30	7	37

Results. The experiment was in the nature of an exploration; the results could not be predicted. I have taken the data as a whole and asked this question: What do the data show about the nature of *P* and *U*? I take up in turn the different attempts made by the *Os* to describe them.

(1) *Bright and dull pressure.* Some of the reports of S appear to confirm a view recently expressed by Nafe that "the affective qualities, pleasantness and unpleasantness, turn out, under direct observation, to be modes of pressure: *pleasantness is a bright pressure, and unpleasantness is a dull pressure.*"[14]

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[12]In the Psychological Laboratory, University of Illinois, from January to May, 1923.

[13]The use of the parentheses was suggested by Jacobson's work (E. Jacobson, On meaning and understanding, this JOURNAL, 22, 1911, 553-578). This technique proved futile in the present case, so the parentheses have been disregarded in treating the data.

[14]Nafe writes: "The *Os* agree that the affective experience comes as a unit, and that only when the pressure described as bright, etc., is present can such an experience be described as *P*, and only when the pressure described as dull etc., is present can an experience be characterized as *U*" (*op. cit.*, 508, 511). This is equivalent to saying that bright pressure is a *sine qua non* of *P* and dull pressure a *sine qua non* of *U*. If Nafe were correct, an analytical attitude should reveal bright pressure every time an *O* feels *P* and dull pressure

"The *P* was spread out from the ticklish sensation. It was bright and thin."

"Later a more intense *P* which was localized around the odor. It spread out in rays and was fluffy and bright."

"*P*; the quality was like weak, light pressure and perhaps such a pressure was at the core of the experience."

"*P* was localized all over the body and was made up mostly of a very weak pressure as I relaxed. There was something besides the relaxation there—a certain smooth, soft texture which seemed to belong to the *P*."

"Moderately *P*. The *P* was soft, fluffy, cloud-like and bright. It was not localized; seemed to be spread out in rays. It was a little like weak pressure or contact."

"The *P* was entirely separate from the cutaneous sensation. It was not localized, but it was very large in extent, very soft and light and thin. It seemed to be in two dimensions only and was bright, almost white in color. It was similar to weak pressure or contact, but it was a disembodied contact."

"*U* which was mixed with actual sensation—dull pressure in the region of the chest and stomach."

"The *U* seemed to be located in the chest and was fused with dull pressure in the chest."

"Very intense *U*, there were meanings carried by dull pressures in the throat which meant that I would hold my breath."

"*U*; then the feeling changed to a different kind of *U* and I thought I should like to cry. This was localized in the chest, and was a dull, heavy pressure and the meaning."

"Moderately *U*; the feeling itself seemed to be made up of a diffuse, unclear pressure or tension all over the body."

P is described as bright, light, fluffy, cloud-like, soft, thin, active, arousing. *U* is described as rough in texture, soft, fused with dull pressure, hard and dark.

It should be noted also that *P* is sometimes reported to be associated with dull pressure, and that there is one 'lively' *U*.

"In the *U* and dull pressure there is something hard and dark; in the *P* and dull pressure there is something moderately soft and bright."

"Feeling of *P* which had its core in a mixture of *P* and dull pressure in the chest."

"The nervousness was made up of a readiness to jump at the least sound or feeling and was muscular tension and a certain kind of *U*—a lively sort."

My reports do not justify the assertion that *P is* a bright pressure and that *U is* a dull pressure. I might conclude that *P* and *U resemble* bright and dull pressures respectively; or else that *P* is *associated with* bright pressure and *U* with dull pressure more frequently than the reverse.

(2) *U and P described as bodily attitudes, especially attitudes of avoiding and accepting.*

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every time an *O* feels *U*; but this is not the case. Further, the associations '*P*-bright pressure' and '*U*-dull pressure' are sometimes reversed. Nafe's failure to consider negative cases may be regarded as a serious methodological defect.

It is interesting to note that Boring, observing in Yokoyama's experiments, reports: "I can't describe *U*. I think it is some sort of organic pattern in my body. I actually believe that it is as much like the dull pressure-aches of nausea as *P* is like the bright contacts of tickle; but it is a far cry from *P* and *U* to these simple sensory things" (M. Yokoyama, The nature of the affective judgment in the method of paired comparisons, this JOURNAL, 32, 1921, 362).

Observer K: "Very *U*. Immediately there arose kinesthetic processes from the face, neck, meaning turning head and screwing up the face to get away from this tickling *U* thing." "The odor was *P*; I liked it. I tried to get more of it and had an attitude which meant 'I wish it were not so weak.'" "*U*; I felt a drawing of the muscles in the face and a sort of jerk of the head to one side away from the stimulus."

Observer G: "A new pattern of organic qualities. A change in the kinesthetic qualities in the chest meaning a *P* odor to be inhaled as much as one can." "Faint kinesthetic or organic processes which meant turning away from the odor." "Perhaps the organic and kinesthetic processes were not *U* but they meant withdrawal, avoidance, expectoration, etc." "A diffuse organic and kinesthetic accompaniment which meant withdrawal, holding of the breath, etc."

(3) *P and U as biological meaning.* The attitudes of avoiding and accepting are sometimes described as kinesthetic-organic patterns (see above), and sometimes the significance of these attitudes is merely stated without analytical description. The two types of report are intertwined; the following excerpts are intended to illustrate chiefly the *meaning* side of the reports.

Observer K: "The *U* meant a desire to have it stopped. There were muscular reactions of avoidance and escape." "When I say this stimulus gave me a *U* feeling I mean that I was affected in such a way that I wanted it to stop. I either wanted to get away from it or have it get away or stop."

"The experience was that of nausea; the feeling was one of repulsion. I wanted to get the thing away, or to get away from it."

Observer G: "Very diffuse and obscure organic and kinesthetic processes in the viscera and chest meaning comfort. An equally obscure and diffuse, very faint, feeling of bodily *P*." "The general bodily set of aversion and withdrawal carried the meaning of *U* more clearly, more intensely." "*P* might be the meaning given by the kinesthetic and organic set which meant 'I'd just as soon continue smelling it.'" "Along with the olfactory quality there was plenty of kinesthesis about the face and chest which meant withdrawal, of a dislike, or dissatisfaction, or discomfort. Whether all those things carried the meaning of *U*, or whether there was a *U* to the odor itself I cannot say."

Observer S: "This *U* may be nothing but the sensations resulting from puckering up the mouth and frowning, plus the meaning that accompanied these." "All the experience this time was organic sensation plus meaning as far as I could observe."

(4) *P and U as mere verbal statement of meaning.*

Observer S: "Part of what I call *U* seemed to be meaning, as if I said to myself 'I don't like that' although I didn't say it or think it, but there was a kind of 'short circuited' meaning having the same reference as if I had said it." "This was tantalizing and interesting and nice, so it must have been *P*."

Observer K: "I had what was probably a conscious attitude meaning perhaps 'Oh, I like this.' There were some fleeting, scrappy images of spicy foods." "This experience might be called *U*. I can't see that there is any special feeling to it." "All that I mean when I say that this is *U* is that I do not especially enjoy bitter tastes. I should rather taste sweet."

(5) *P and U as characteristics of the object.* In the foregoing reports the observer, in a sense, has looked inward for a description of the affective processes. *P* and *U* are regarded as components of individual experience which resemble pressure,

or as the kinesthetic-organic equivalents of the attitudes of accepting and avoiding, or as the meaning of these attitudes. It is a question whether the following reports really characterize *P* and *U* as such. They refer *P* and *U* to the object and regard *P* and *U* as though they were bound up with the object.[15]

Observer G: "The *P* seemed to be part of the total character of the cutaneous pressure and localized with it." "The *P* quality is a part of the perception of the odor." "The *P* feeling was an integral part of the tone experience and located with the tone." "I could not detect two things, the pain and the *U*. The experience was just that kind of an experience, *viz. U*." "The odor was distinctly *P* and the *P* seemed to belong to the odor as part of its total character." "The *U* was a distinct part of the total perception." "The *P* was a genuine part of the odor so far as I could tell." "The *U* characterized the odor itself." "The *P* seemed to be a part of the perception, a characteristic of the tone itself." "A *P* odor. The *P* was a part of the perception."

Observer K: "It seemed to me that the *U* quality was inherent in the sensation of bitterness. I just did not like the bitter taste and that is about all I could observe." "This olfactory sensation gave me a feeling of *P*. I seem to identify the feeling with the sensation. It just seems that the sensation *is P*. It is the kind of a sensation that I should like to have more of." "There was pain which was *U*, localized on the forehead. It seems in this case that the feeling of *U* was tied to the sensation as an attribute."

General Discussion

(*A*) *The sensorial attitude.* In the physical sciences observation involves the direct stimulation of a receptor organ. It involves a predisposition, a set of the *O* which is equivalent to the purpose of the observation.[16] It involves or implies the presence of a phenomenon in the *O*'s field. Frequently observation is followed by report, in words or in other symbols.

In such sciences as chemistry, geology, astronomy, etc., the essential conditions of a particular observation lie outside and beyond the organs of sense. Consequently the physical sciences have regarded the *O* as a troublesome source of error and the physical methods have heretofore aimed at his elimination. The analytical psychology of datum has regarded sensory processes—pressures, reds, tones, bitters, etc.—as directly dependent upon the stimulation of receptor organs and the bodily structures behind them. Observation in sensory psychology has been essentially the same as observation in chemistry. The chief difference between a psychological observation and a chemical observation lies in the problem which is presupposed. In the study of mental imagery the sensorial type of observation has been found to be fruitful of results.

In approaching affective psychology one may take up the sensorial attitude and ask the question 'What?' 'What is *P* as

[15] Cf. Nafe, *op. cit.*, 538 f.
[16] P. T. Young, The phenomena of organic set, *Psychol. Rev.*, 32, 1925, 472-8.

given?' 'What is *U* as given?' The 'What?' attitude, consistently maintained, should yield all the 'Thats.'

One difficulty in answering the question 'What?' appears as soon as one attempts to observe directly and independently those characteristics of experience called 'pleasant' and 'unpleasant.' It is commonly maintained among psychologists that *P* and *U* lack clearness. When one attempts to observe directly a *P* or a *U* one finds only sensory processes. *P* and *U* are elusive; they are not directly observed.

The reason for this is doubtless the fact that the essential conditions of *P* and *U* are not in the stimulus nor in the receptor organ, but functionally behind the receptors. As evidence for this statement I note briefly: (1) The stimulation of any receptor or any group of receptors may yield an experience characterized as *P* or as *U*. No single sensory structure appears essential to the existence of affective processes. (2) With the pattern of stimulation remaining approximately constant experience may shift from *P* to *U*, and sometimes the shifts are rapid. (3) Experiences which are generally believed to be centrally conditioned—the experiences of memory, imagination, thought—may evoke intense affective processes.

The attempt to approach affective psychology from the sensorial point of view has this consequence: whatever is observed is sensory. Thus Nafe, assuming *P* and *U* to be 'palpable' found bright and dull pressures.[17] The sensorial attitude makes these pressures sensory in nature, however obscurely localized and ill-defined they may appear. The sensorial attitude yields something sensory, or nothing.

In the present experiment when the *O*s took the analytical point of view towards experience they observed qualities in the cutaneous-kinesthetic-organic group. They observed sensory patterns which *meant* avoiding or accepting, etc. Also they observed gross objects and characterized them as *P* or *U*.

(*B*) *The functional report.* My *O*s regarded *U* as the meaning of avoidance and *P* as the meaning of acceptance. Such statements are not analytical observations but functional reports. Following Bentley's terminology, we may distinguish the analytical *inspection* from the making of comments.[18] The *commentary* report may be illustrated as follows:

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[17]Nafe might object that the term 'palpable' is not restricted to sensorial observation. To this I reply, (1) if the term means more than sensorial observation, it is introducing still further confusion to affective psychology; and (2) as far as I can see the *O*s actually did take a sensorial attitude and they described in terms of pressure which is essentially a sensory process. The sensorial attitude in affective psychology has been discussed by Yokoyama. Cf. M. Yokoyama, Affective tendency as conditioned by color and form, this JOURNAL, 32, 1921, 88 f.

[18]M. Bentley, *The field of psychology*, 1924, 41-46, 197-199; The major categories of psychology, *Psychol. Rev.*, 33, 1926, 88-91.

After smelling a bottle of putrified fish the *O* replied: "I don't like the odor; it is very unpleasant; take it away." During the experiment I noticed that he was holding his breath and that he turned his head away from the bottle.

When the *O* reported "I don't like it" was he observing? The discussion has made it clear that he was not analysing, not inspecting, data from the sensorial point of view. His comment is descriptive of something; it is easily and generally understood; it is psychologically significant. The report 'I don't like it; it was *U*' is just as significant as the statement 'I feel a bright pressure.' The latter statement implies a sensory process; the former implies a dislike which is not directly observed as a sensory process, but which is merely reported unanalytically.

It is not only legitimate but scientifically worth while to assume the sensorial attitude towards the problems of affective psychology and to go as far as one can go with it. There are numerous problems which may well be approached in this way. I believe, however, that psychologists should also accept at face value the functional, commentary, unanalytical type of report. To accept this type of report as of genuine psychological importance opens the door to a non-sensory psychology wide in its scope.

The functional type of report furnishes the only evidence I know for the existence of affective processes. *P* and *U* are not directly observed from the sensorial point of view. Perhaps the most that can be said is that *P* and *U* are directly *felt*, that they are ultimate non-sensory data, and that the comment 'I like it; it is *P*' is a direct expression of the *felt* experience.

That pleasant and unpleasant experiences exist can readily be demonstrated. I believe that the average person upon smelling the putrid fish would express dislike in some way or other. The terms *pleasant* and *unpleasant* significally characterize gross experiences.

The functional report, it should be remembered, is not sensorial, not even analytical. If the question 'What?' must be answered, we may either (1) accept whatever a sensorial attitude yields, or (2) accept the functional report at face value and *infer* the existence of non-sensory processes of experience.

Incidental Results

The following incidental results have come out of the experiments upon affective localization and the nature of the affective processes.

(*A*) *Results from a synaesthetic O.* S describes *P* as bright, light, almost white; and *U* as dark, dark gray, black. In the same context S also describes *P* as light yellow; and *U* as greenish yellow, slightly reddish, dark brown, brick red. The clearest examples are given below.[19]

[19] A similar case has been described by Cutsforth. "In summary, pleasantness as an accompaniment to vision consists of reds, blues and yellows, and as an accompaniment to audition of rose-reds, blues and brighter yellows. Unpleasantness, as a visual accompaniment, consists of dirty browns and dull greens; as an auditory accompaniment it consists of dirty browns and less saturated, dead greens." (T. D. Cutsforth, The rôle of emotion in a synaesthetic subject, this Journal, 36, 1925, 536 f.)

"The *U* increased in intensity and extent. It was a dark greenish yellow color with a smooth compact texture."

"The *P* was soft and fluffy, loosely woven in texture and very light in color, a sort of light yellow. The core was a little darker."

"Either the pain or the *U* was dark gray, almost black in color."

"The *U* was decidedly black in color."

"There was a tendency to withdraw and the tendency plus a blackness seemed to constitute the feeling."

"This *U* was followed by anger. This was localized in the chest and was like dull pressure and warmth. It seemed to well up, increasing in intensity and extent. It wasn't dark like *U*, but was brick red in color."

"*P* was bright, almost white."

(*B*) *Waves and other movement of sensory feelings.*[20] Three *O*s (K, S, W) report waves of feeling which move through the body, especially in jar, shock, or surprise. K and W are very sensitive to these moving experiences, and K reports them frequently.

Observer K: "What I felt was a wave of sensation which seemed to start about the ear and to spread as far as the belt. The wave was on the order of a succession of weak thrills" (noise). "I'd say that was *U*. I felt a thrill or wave going all through my body, legs, and arms. It was much farther reaching than any so far. I felt the thrill in my finger and even down to my legs" (sandpapered chin). "I felt a wave go all over me, down to the toes and hands—a wave of *U*, a *U* shock or jar. It was down to the toes and fingers before I realized it. It originated somewhere in the upper part of the body. I observed it going down the trunk and legs. It traveled fast and with a motion outward toward the toes and fingers" (odor of old cheese). "That may be characterized as *U*. What I felt was in the nature of a shock. A thrill or pulse of sensation spread through my body. I not only felt the wave or shock but after the first violent shock there was a tingling sensation in my legs. This was noticed in my knees and arms" (snap on face with rubber-band). "I felt a thrill go clear through me, down to the legs, a catching of the breath, a jerk of my body. It was *U*" (slap on the face).

The 'waves' move downward and outward, *i.e.* in the peripheral direction. Their localization is not very precise but their movement can readily be observed. The 'waves' move through the gross regions of the body and can frequently be followed to the fingers and toes. They are called forth by auditory, olfactory, tactual, and other stimulation. They are observed as sensory in nature but as regards definiteness of localization, movement, periodic character (tingling, etc.) they differ from the more familiar sensory processes.

In the above examples the experience is reported as *U* whenever the affective quality is specifically mentioned. The existence of *P* waves, however, is suggested by the following report:

"A tingle in my legs—more in the nature of a tingle than in the soft waves, as with *P* stimuli."

Observer S: "The feeling was very *U* and was accompanied by waves of dull pressure that travelled over the body, and muscular tension in the head and back."[21] "*P*; it was very much like waves of weak pressure travelling over the body, but the waves were not localized in the body but out in space." "*P*; there were waves of pressure beginning in my nose and

[20] So far as I am aware these spatially moving feelings have never been adequately described. I have previously noted their existence, this JOURNAL, 29, 1918, 427.

[21] In view of Nafe's conclusion it is interesting to note that S describes *U* 'waves' in terms of dull pressure.

running over my mouth and chest to my stomach." "Moderately *U*; then waves of pressure ran down the body and it became very *U*."

Observer W: This *O* is extremely sensitive. She reports a good many feelings which come in waves, sweep through the body, run through the body in a definite direction, spread, move up or down the arms and back, moving especially in waves or shivering. Examples are: "I had a distinct feeling of nausea which came in waves and was quite extensive. It didn't seem to be localized. It came in waves and receded." "Beginning in the region of my stomach and sweeping upward to the throat, as nearly as I could locate it, was a feeling which I could characterize as neither *P* nor *U*." "A quick jump with a certain amount of hand movements. A sharp feeling running through, almost a pain running from right shoulder in a diagonal way to my heart. Feeling takes on the characteristics of a line, a small area and a straight line shooting down. I can't catch enough of it to know its quality." "In the region of my stomach very quick feelings of nausea. Chills up and down my arms and back. They moved down from the throat regions in waves; they didn't come back." "I felt waves of something up and down my arms, not exactly chills, waves of feeling and I don't know how to describe that feeling." "A distinct *U* sensation. It sent all sorts of little waves of feeling rapidly all through my body. My right foot was tapping in annoyance while I was trying to swallow the liquid. A general bodily disturbance, the most marked of any I have had. I think there was a coldness in the creepy feeling, a difference in temperature from the temperature of my skin. I wasn't sure whether the creepy feeling was colder or what." "A definite feeling down my throat and into the stomach. Waves of nausea. Shivers down back and arms. I puckered my mouth, shook my head. Uncontrolled shivers through arms and spine and in a straight line through stomach. I didn't seem concerned that the taste was in my mouth."

The complete description of these moving bodily feelings would constitute an interesting problem in analytical psychology.[22]

(*C*) *Strain and relaxation in relation to P and U.* In a previous paper, I pointed out an association of *U* and strain, and of *P* and relaxation.[23] The present data confirm the association.

Association of P and relaxation. Examples of the association of *P* and relaxation are:

Observer K: "The experience was restful, quieting, soothing, relaxing —*P*. There was some kinesthesis in thoracic viscera and throat (meaning effort to draw in the odor to get more of it)." "This experience was *P* to me. In this I experienced a tension of the muscles just before the stimulus was given (this was suspense and expectancy). With the stimulation came a relaxation, a comfortable and agreeable feeling. It was an experience to be desired, something I should like more of." "Very *P*; there was something soothing and relaxing in the experience. It seemed soft and comfortable and there was something in the quality of the odor which I liked and which seemed to set me in a relaxed and surrendered bodily state. The agreeableness of the bodily state was observed throughout the thoracic viscera especially. There was some vague and obscure imagery which meant flowers and beautiful things in general." "I felt myself relax as the

[22] If any one contemplates making such a study, it should be remembered that individuals differ greatly in the frequency with which they report these experiences. The investigation should be carried on from the sensorial point of view.

[23] P. T. Young, Pleasantness and unpleasantness in relation to organic response, this JOURNAL, 32, 1921, 40-44.

tones continued and as this went on the *P* seemed to increase and I observed then some light pressures in the chest which seemed to add to the agreeableness of the total situation."

Observer G: "The odor was soft—*P*. A sudden dropping out of organic and kinesthetic processes. General bodily relaxation, diffuse feeling of comfort."

Observer S: "I experienced a distinct relaxation and this was or meant *P*."

Association of U and strain. Examples of the association of *U* and strain are:

Observer K: "This experience was *U* to me. I was in a fairly relaxed condition just before the stimulus was given. When the stimulus was started there was a sort of tightening of muscles, a sort of an attempt to brace myself against this noise." "The *U*, as far as I am able to discover, seemed to lie in the muscular response. I felt a very decided contraction of the muscles of the face and especially of the eyes and region about the forehead."

Observer S: "Very *U*. The *U* was fused with pressures which shot back and forth through the chest and head. A great deal of tension in the muscles which meant that I wanted to draw away from the stimulus." "Very intense *U*, the kind that seems unbearable. Accompanied by muscular tensions all over the body." "A strong desire to pull away which was carried by muscular tensions." "The *U* was made up of shrinking or muscular tension." "A great deal of tension of the muscles which meant that I wanted to draw away from the stimulus. There were streaks of greater *U*, which appeared at times in the complex of *U* and pressure."

I might point out, as I have previously done, that the correlation in question is by no means perfect. Frequently there are strains—especially those involved in surprise, attentive observation, anxiety, fear and other attitudes—present in indifferent and in *P* experiences. There may also be *U* relaxations, although I can find no evidence for them in my data.

III. The 'Trained' Observer in Affective Psychology

In view of the present confusion in affective psychology it was deemed advisable to make a minor experiment bearing upon the methodology of analytical psychology in the sphere of affection. If one control the stimuli and other important factors in an experimental situation, and if one control the instructions and suggestions presented to the *O*s, will practice (in the sense of repeated observation) bring a group of *O*s into approximate agreement regarding the critical point of the experiment?

I decided to put the question to a test using Nafe's recent experiment and limiting the problem to his chief conclusion, namely that: "The affective qualities, pleasantness and unpleasantness, turn out, under direct observation, to be modes of pressure: *pleasantness is a bright pressure, and unpleasantness is a dull pressure.*"[24]

In the following experiment I have endeavored to duplicate Nafe's experiment, using his stimuli, his instructions, and reproducing so far as possible the total experimental situation.

[24] J. P. Nafe, An experimental study of the affective qualities, this Journal, 35, 1924, 508.

Observers. I selected 3 *O*s: Dr. Elizabeth Möller (M), instructor in psychology; Dr. E. A. Esper (E), assistant professor of psychology; and Miss Lillian Nilson (N), undergraduate student majoring in psychology.[25] M, who holds the doctoral degree in psychology from Cornell University, has been impressed by the Titchenerian creed. E completed his doctorate in psychology in Ohio State University. He is of the behaviorist's faith. His views upon the aims, problems, and methods of psychology differ from the views of M as widely as black differs from white. N is approximately neutral as regards psychological education. She is a junior at the University of Illinois, a normal, competent individual who is quite innocent of theoretical views upon introspection, feeling, and other psychological matters. All the *O*s took the work seriously and faithfully.

Stimuli. The following stimuli were used in the experiment. The 'catch-word' which represents them appears in parentheses.

Visual: (colored strip) a series of colors painted on a strip of paper, mounted on a card, representing the complete series of hues, exposed in moderately bright artificial light for 5 sec.

Auditory: (chord) four forks, C, F, A, C′, struck in the order given with a very light blow. The chord was allowed to continue about 3 sec., and the forks were then damped in the same order; (loose fork) a fork mounted loosely to the resonator so that when struck rather violently it produced a rattling, noisy tone. It was sounded from 1 to 3 secs. In addition to the above, which were used by Nafe, I also used the chord C, E, G, C′, and the noise of filing glass close to *O*'s ear.

Olfactory: (presented in bottles) oil of peppermint, old fish, caryophyllorum, stale cheese, menthol, nutmeg, fish oil, white rose perfume, anethole, phenol, pyridine, camphor, methyl salicylate, ethyl cinnamate, geraniol, oil of mace, napthaline, nitrobenzole, carbon disulphide. The *O*s were instructed to breathe normally. The time of exposure was the time of one inhalation, but occasionally two or three inhalations were permitted. These odors are in large part different from those used by Nafe; but for purposes of the experiment they are equivalent in that they are capable of arousing affective experiences of moderate intensity.

Gustatory: vinegar, salt solution, concentrated sugar solution, sarsaparilla. A few drops of the latter were placed on the tongue by means of a spoon. One drop of the other solutions was placed on the tongue by means of a camel's-hair brush.

Tactual: (hot steel) a piece of steel, about 1 in. in diam. at the contact surface, heated to 50°C or above; (cold steel) a similar piece of steel cooled to about 10°C. These stimuli were applied to the volar surface of the forearm for 3-5 sec.; (hair) a moderate-sized camel's hair brush drawn two or three times across the forehead or lips; (glass) a smooth piece of glass moved over the tips of the fingers; (sandpaper) a piece of No. 0 sandpaper moved over the tips of the fingers, the chin or the nose; (eraser) a felt eraser, filled with chalk dust, moved across the tips of the fingers; (nail) a sharp nail pushed slightly into the volar surface of the forearm to give moderate pain; (string) a piece of heavy string with frayed end which was moved across the closed lips of *O*.

A comparison of the above list with that of Nafe reveals that, apart from the olfactory group, my stimuli are almost identical with those used by him.[26] In the olfactory group and in the case of a few additional stimuli the two sets, Nafe's and my own, are equivalent for purposes of evoking moderately intense affective experiences.

The method of presenting the stimuli was the same in both experiments except for the following inconsequential differences: Nafe did not hold his

[25] Nafe also employed an undergraduate as a control (*op. cit.*, 510).
[26] *Op. cit.*, 509 f.

Os to any fixed number of reports at a particular sitting, whereas I obtained a series of 8 reports at every experimental sitting regardless of the time consumed. Every series began with 2 odors and ended with 2 odors, and the remaining 4 stimuli were selected from the tactual, auditory, gustatory, and visual groups. I made much use of odors on account of their well-known capacity to evoke *P* and *U*. No stimulus was used twice during a single experimental sitting.

Procedure. The experiment was done during the fall of 1925. Every *O* observed twice a week for 5 weeks. As 8 reports were taken at every observational sitting, a total of 80 reports were thus obtained from every *O*. The work for a given *O* was done at the same hour of the day throughout the period. The time of the sittings varied from 20 min. to 1 hr., according to the length of the reports.

The *O* was seated in a comfortable chair which was enclosed in a 7×9 ft. booth curtained off for the purposes of the experiment. The stimuli were kept on tables outside of the enclosure. The sound of *E*'s approach served as a warning signal. *O* kept his eyes closed during the stimulation, except when visual stimuli were used in which case he opened his eyes at *E*'s request. The verbal reports were recorded literally as given, and are given below unedited as they appear in the protocols.

Instructions. At the start and also at the close of every experimental sitting *O* was asked to read the instructions. This was done to impress *O* with his observational task and to make certain that the instructions would be a determining condition of his reports. The instructions were taken from Nafe.[27] They are:

"I shall give you a stimulus which is intended to arouse a moderately pleasant or a moderately unpleasant sensory experience. You are to direct your attention as exclusively as possible to the affective side of the experience. When you judge that the feeling is at its maximum, break off your observation and describe the feeling itself as accurately as you can."

This instruction was supplemented according to Nafe by the following:

"Continue the effort to get affection palpable under observation for itself. Look for concomitant organic sensations with a view to determining whether affection is simply a meaning laid over these organics."[28]

The first instruction was used for the first 24 reports. Both instructions were used together for the next 16 reports, and the second instruction was used alone for the last 40 reports. I am not able to discover exactly how Nafe made use of the two instructions.

Results

For illustrative purposes I have selected one report from the protocols of every *O* for every day of the experiment. I selected the fullest and most complete, and present them unedited, just as they appear. The order in which they are presented is that in which they were given. The number in parentheses indicates the serial position, the 'catch-word,' gives the stimulus used.

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[27] *Op. cit.*, 510.

[28] When handed these instructions N asked to have certain words explained. My explanation was off-hand as the question was not anticipated. The words and the explanations as given are here recorded: (a) *Affection*: this term refers to pleasantness and unpleasantness and possibly to other feelings. (b) *Palpable*: this literally means 'touchable,' i.e. observable. The point is whether you can observe feelings of *P* and *U* directly *as* feelings. (c) *Concomitant*: this means present at the same time. (d) *Organic sensation*: the term refers to pressures, aches, warmths and other sensations localized in or referred to the body. (e) *Meaning*: I can't fully explain this term. It is the same as 'significance,' and the word is used here as in every day life.

For the sake of uniformity I asked M and E whether they understood the instructions and they replied that they did; consequently no explanation was given. Both, however, complained of difficulty in carrying out the instructions: M about the instruction to "break off the observation when feeling is maximum;" and E about the instruction to "look for concomitant organic sensations."

OBSERVER M

Reports: (3—camel's hair brush) The experience was *P* and that *P* seems to be a pressure. It isn't a pressure on anything, and it's a pressure that is light gray. I don't like to say it is gray and yet gray is the nearest thing it is like. This pressure is unlocalized. That's what I mean when I say it is not on anything and it seems to be just all of experience for an instant or two. I can't point any place and say, 'That's where it is' because it isn't that kind of an experience.

(13—noise) That was *U*. There seemed to be a big gray band of experience, and when the *U* comes in it comes against this background. In this case the *U* part was rather heavy, compact, a dense thing that is much like pressure. It has nothing visual about it and yet I want to say it is almost black. The pressure is just everywhere, not localized any place and the black doesn't appear any place.

(22—C, F, A, C′) When *P* appeared it seemed to be like a light, steady pressure. It is something that is big or that has volume as a cloud has. This wasn't a volatile, dancing pressure, but one that was smooth and soft and steady. It isn't a pressure at any place, but it exists and seems to exist as air does. There was a definite coloring in this one. This pressure seemed to be in a gray band. When I'm talking about the background I am never there as looking at it. It goes on.

(30—noise) That was *P* but it should have been *U*. The *P* part was bright; it was springy, buoyant, and the little pressures went up. Made up of lots of light pressures; they were in motion all the time. They were bubbling up, and yet they don't bubble any place.

(35—sandpaper nose) Slightly *U*. The *U* was sort of heavy, like a dull pressure as opposed to a bright pressure, a dead heaviness. That was noticeable in this case because the experience was rough and yet the *U* wasn't rough itself. It was smooth. I didn't notice any organics. The *U* seems to be the whole bit of experience. There doesn't seem to be anything else there except the carrier of the experience.

(44—camel's-hair on palm from wrist to finger tips) Slightly *U*; the *U* is dull. I think that is the most striking difference between *P* and *U*. The *U* ones are all so dull. It wasn't like a heavy pressure. More like a dull pressure than anything else. There is a kind of solidity about it which the *P* ones don't have. The *P* ones are like film but this one is more like a surface. Yet the film and surface aren't any place. No organics noticed. In this case it was quite obvious that the *U* didn't arise till after the cutaneous experience had gone on a little while.

(55—geraniol) Moderately *U*. The *U* is something that goes down. It was dense and dead, most like a dull pressure although it wasn't a pressure any place. It was unlocalized except that it seemed to be everywhere, but yet there was a background for it. I didn't notice any organics. As soon as the odor went the affection was gone too.

(59—C, F, A, C′) That was *P*. The affective experience in this case ran a certain course. The first auditory experience was itself *P* and the *P* was springy, volatile, something like gray and something like pressure, without being either gray or pressure. As the other auditory experiences (tones) came they somehow mingled with the first *P* and the experience was then more uniform. It wasn't quite so bubbly and springy. It was, however, something light that pressed up rather than down, and it was gray. Somehow it was a light gray. After the experience had gone on for a while I broke it off to look for organics. Of course I found some but they weren't related in any way to the affective experience.

(68—noise) That was moderately *U*. The *U* is just as palpable when it comes as the auditory experience itself. It rises to a maximum and seems to dominate all of experience so that other things drop back to a low level.

The *U* was heavy, dull, inert. I think I get its dark meaning from the heavy clouds of smoke and from dull, black bits of metal. It is really like a dull pressure only it isn't a pressure any place.

(75—colored band) That was moderately *P*. The *P* is soft and it seems to be light and to push up instead of down. It is more like a film than anything else. It is misty and there is something bright about it. I didn't notice any organics.

Questionary. At the close of the experiment M was asked a few questions, a record of which appears below:

Q: Were you an observer in Nafe's experiment?

A: Yes.

Q: Have you read Nafe's published report of his work?

A: I am not sure whether I have read it or not. [Note: as this appeared only one month prior to the present experiment it is not likely that M had read the report. If read, it did not leave much of an impression.]

Q: Will you give me briefly your theoretical views regarding *P* and *U*?

A: I think they are real qualities which are just as observable as visual or auditory qualities, and that they can't be described adequately because they are ultimates. They are alike in lots of ways. For example, both *P* and *U* seem to be like pressures and sometimes they have a reference to gray. *U* is smaller than *P* experience and it is more compact while *P* is diffuse. *U* is like a pressure down and *P* like a pressure that is springing up. *U* is dark when it has a color quality there. *U* is like a dark gray and *P* is like a light gray.

Q: Has the experiment changed your theoretical views?

A: My view has been changed in that I am uncertain about that color business, where it is or if it is. I did have a bias at the start because I think that ultimately everything will reduce to pressure.

Q: What, in your view, is the purpose of this experiment?

A: I think it is first of all to determine whether affection can be observed and then to get a description of affection.

Q: Do you think this experiment is comparable to Nafe's, or do you notice any differences?

A: The first instruction "break off your observation and describe the feeling"[8] bothered me all through because I couldn't observe affection and at the same time note when it reached a maximum. Outside of this I don't remember any difference between the two experiments. [Note: The present instruction is word for word the same as indicated by Nafe.]

Q: What do you think about the instructions?

A: I think they are all right except for the above point. In looking for concomitant organics you can't break off the experience and be set to notice organics.

Q: Do you have any criticisms on the experimental technique?

A: No.

Q: Where did you get the hint that *P* and *U* are like pressure?

A: It just came out of a clear sky. All of a sudden I had this peculiar experience, this pressure that wasn't any place, which seemed to exist absolutely independently of me. I don't think it was suggested because I remember the shock I had when I observed it first and from that moment dates my bias towards pressures.

Discussion. An impartial reader of M's reports would gain the impression that M is struggling with words in an endeavor to describe *P* and *U*. *P* is something bright, light (in the sense of not heavy), bubbling, rising up, billowy, sparkling, volatile, airy, springy, diffuse, alive, dancing, smooth, soft, buoyant, like clouds or like smoke, made up of lots of light pressures, penetrable, misty, like film, active. *U*, on the other hand, is something dull, dark, dead, heavy, small, compact, black, rough, grainy, like gray, dense, thick, pushing down, like dark or heavy smoke, inert.

The contrast between *P* and *U* is expressed in the frequently used pairs: bright-dull; alive-dead; rising up-sinking down; active-inert.

The affective processes together are said to be 'like pressure' or 'like gray.' I doubt whether M's reports justify the assertion that *P is* a bright pressure and *U is* a dull pressure; but there is no doubt that the reports are fairly consistent in stating that *P is like* a bright pressure and *U is like* a dull pressure. Also *P* and *U* are similar to visual brightnesses.

If the list of terms, descriptive of *P* and *U*, be compared with Nafe's list,[29] and especially if M's former and present reports be compared, then it appears that the present experiment is a rough confirmation of Nafe. Further, M herself stated, as shown above, that she noticed no marked difference between the experiments.

Observer E

At the close of every sitting, except the first, E was asked: Are there any comments you would like to make about to-day's experiment, or about the experiment as a whole? These comments are the most illuminating part of the record and appear below as 16A, 24A, 32A, 40A, 48A, 56A, 64A, 72A.

Reports. (6—C, F, A, C′) I was awaiting with pleasurable anticipation the addition of further tones. I derived considerable *P* when I heard them all together. I was disappointed when they didn't drop off in the same uniform manner as they had been added.

(9—carbon disulphide) I was expecting that glue smell, so the first thing I got out of it was that it wasn't so bad as I expected. As you went away 'cooked onions' occurred to me. [Note: O turned his head away from the bottle.]

(16A) It isn't clear to me what problem I am to set myself. If I ask myself, 'Is this *P* or *U*?' I could probably answer it one way or the other each time. If I took Wundt's system, I could probably answer in the six ways. I myself am not convinced that there is anything but momentary significance to it. There is practically nothing I can say about the experience as such.

(18—phenol) Again rather *P*. Weighted with a lot of associations of childhood experiences, various occasions in which carbolic acid entered.

(24A) I find I don't have much success in carrying out the instructions. There doesn't seem to be much affection to turn my attention to.

(26—stale cheese) Again the response released, in spite of instructions, is an identifying response. I am quite unable to report any affection other than avoidance. Imagery of decayed cheese. [Note: O turned his head away.]

(32A) It appears to me that it is quite impossible to carry out instructions because the stimuli are so indifferent. The only behavior released is an identifying behavior and possibly then a withdrawing. I don't find anything to report except in some cases *P* or *U*, but I am not convinced that those reports mean much more than a recording of my avoiding reactions or non-avoiding reactions. The terms *P* and *U* don't stand for something more than those avoiding reactions. It is a bare verbal report which means nothing more.

(39—fish oil) Well, that was perhaps more interesting than the others. If I had been left to myself, I would have continued smelling it longer to identify it. I wasn't sure whether it was agreeable or disagreeable.

(40A) When I say 'pleasant' it doesn't stand for anything more than 'I would smell it more if I could.' I am not able to discriminate anything beside that in most cases.

(43—salt) Merely identification as salty. I might add, perhaps *P*, but that would be merely a verbalism. It doesn't stand for anything else.

[29] *Op. cit.*, 517.

The response 'pleasant' pops out as in an association experiment. Organic sensations simply don't enter in at all. I seem to be doing business exclusively at the upper end of my body.

(48A) I find this difficulty. The instruction asks me to watch for organic sensations. That means that I have a tendency to respond to various conditions in the abdomen which are present all the time but to which I don't ordinarily respond. When I read the instructions I begin responding to them. I have difficulty in knowing whether I am reporting something as from the stimulus or from other conditions after stimulation. An utter blankness as to what to report. Usually there is nothing except stimulus is so-and-so or suggests so-and-so.

(52—colored band) Well, I identified it as a spectrum and perhaps found some pleasure in inspecting the colors although I don't know what that means when I make use of that expression except that it is the verbal equivalent for continuing to look.

(56A) The judgments of *P* and *U* are merely verbal symbols for movements towards or away from the stimulus, continuing or avoiding. Such questions as 'Why is it *P*?' or 'What is it?' release no further response than, for example, 'The odor is *U* because it stinks.'

(63—anethole) A suggestion first of an association with some sort of medicine. Perhaps a slight nausea of the sickly sweet type again, although I am not at all certain of that. At the present instant I can discriminate nausea. I think the sequence is a verbalism 'sickly sweet' that reminds me of the instruction about organic sensation, and that leads me to make abdominal discriminations which are likely to be of the order of nausea. It happens that my digestive processes are not perhaps what they should be to-day.

(64A) I find the instructions very vague. When you ask me to report on my affections, or my feelings, it isn't at all clear to me what that means specifically except perhaps that I can verbally report that this or that is *P* or *U*, or that I can report that one thing is more *P* than another.

(71—nutmeg) Very interesting and *P* odor. Probably I would continue investigation of it for some time if left to myself. Vague spicy suggestion and other factors which I didn't have time to analyse out.

(72A) It has occurred to me that in most of these stimuli if you consider my probable responses under ordinary conditions, none of them last long enough to be sure whether I would avoid or continue listening. I don't get past the point of finding out what the situation is in most of them. Take, for example, those cheese odors. One whiff is enough; I would probably get away. Even in case of a given stimulus when I report *P* possibly a little bit more of it would make me respond by getting away. I hardly get past the stage of a hasty identification of the stimulus which usually consists of a few associations.

(75—colored band) Well again subvocal *P*, warm, cheerful. A rather active examination of the stimulus field and had the stimulus continued I'd have settled back and looked at it with interest. I had just about got adjusted to it when the stimulus was removed. I was at first looking too high. It took me an instant to find the stimulus.

Questionary. At the close of the experiment E was asked a few questions. A complete record of questions and answers is given below:

Q: Are you familiar with Nafe's experimental study of the affective qualities?

A: No.

Q: Are you acquainted with Titchener's touch pyramid?

A: No.

Q: Will you state briefly your theoretical views regarding pleasantness and unpleasantness.

A: My views on *P* and *U* would coincide with those of Max Meyer in the *Psychological Review* about 20 years ago in which *P* is regarded as

facilitation of behavior, and *U* as interference.[30] With respect to the present experiment, if anything, I have been confirmed in my belief in the adequacy of that view, although as a matter of fact that didn't occur to me till to-day.

Q: What do you think is the aim of this experiment?

A: My answer would be colored by my knowledge of the subjects. You told me who your subjects were.[31] I assume that the experiment has as its object the comparison of data obtained from individuals with different types of training. Perhaps this is a study of the validity of introspective reports in connection with feeling experiments. I have assumed that if you wanted a mass of introspective data on feeling you would get some introspective experts rather than myself and one other naïve individual.

Q: Do you think your suspicions about the nature of the experiment influenced your reports?

A: No. I was trying to follow instructions.

Q: What do you think of the instructions?

A: The part about 'organic sensation' annoyed me very much. I seemed to be doing business with the upper part of my body and I had to identify and this took most of the time. There wasn't time for other types of discrimination such as organic. I was also disturbed by paying attention to the abdominal region rather than to the stimulus. When I was discriminating what was going on in the abdomen I wasn't responding to the stimulus.

Q: Do you have any criticisms of the experimental technique?

A: No, because I don't know its purpose.

Q: Is there anything further, in general, to add?

A: No.

Discussion. E's reports do not give a hint of anything comparable to the statements made by Nafe's *O*s. Not once are *P* and *U* described as pressures, as similar to pressures, or as associated with pressures. Not once do the terms 'bright,' 'dull,' or their equivalents, occur. Why has E failed so completely to confirm Nafe's conclusion?

Taking E's reports at face value, one finds therein a fairly consistent view of *P* and *U*. 'Pleasant' and 'unpleasant' are verbal responses, and as such *P* is the equivalent of letting the reaction continue and *U* is the equivalent of an avoiding reaction. At the close of the experiment E stated his theoretical views upon *P* and *U*. So far as I can see, E's reports demonstrate or 'prove' his view to be 'correct.' The view in question can be referred to E's training in psychology at Ohio State University.

M also, at the close of the experiment, expressed her views upon affective processes. So far as I can see, M's reports and her theoretical views are consistent. M's views are derived from psychological education at Cornell University.

It appears, therefore, that within the limits of practice set down in the present experiment, mere repetition of observation under controlled conditions has failed to bring two *O*s with radically different psychological education into agreement. Each *O* reports in a manner which is fairly consistent with his own theoretical views.

At this point let me anticipate certain objections which some of my readers will raise.

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[30] M. Meyer, The nervous correlate of pleasantness and unpleasantness, *Psychol. Rev.*, 15, 1908, 201-216, 292-322.

[31] At the close of the experiment on Nov. 19 (after report 48A) E asked: "How many subjects do you have?" I replied, "Three—Miss M, yourself, and a student." E then said, "It is a clever idea to use such different subjects; you are really working on observation, then, and not on feeling. I replied, "This is a study in feeling." E added: "Excuse me for anticipating; we'll talk it over when you are through!"

Although E had guessed the purpose of the experiment at this date, I doubt whether this guess influenced his reports. His data are uniform throughout.

(1) E's reports are 'common sense,' '*Kundgabe*,' 'mere casual comments.' If such terms be used to describe a specific type of report and to distinguish it from other types, I see no objection to them. If such terms be used for purposes of wholesale condemnation—as they frequently are—I object to this method of clearing up the situation. To throw out all of E's reports as '*Kundgabe*,' or as something else equally terrible, is to avoid the problem. One would be equally justified in throwing out all of M's reports on the ground that they are 'poetic,' 'literary rather than scientific,' 'over-imaginative,' 'biased by psychological preconceptions,' etc. Either we should discard both sets of reports and thus avoid the problem, or else we should face the problem squarely.

Every report of E, without exception, is straightforward, understandable, clear-cut. Literary style may be criticised but this is wholly beside the point; all printed reports are raw data, just as they were dictated in the laboratory.

(2) It may be objected that E failed to carry out the instructions.

The fact is that E understood the instructions and with good will carried them out to the best of his ability. There is good evidence that the instructions essentially determined the nature of E's reports.

It is true that E did not talk at great length about *P* and *U* as qualities of experience, but in this particular experiment the question at issue is whether *P* and *U* are 'palpable.' Possibly E did not observe *P* and *U* for the simple reason that he found nothing affective which was observable; he found only reactions, etc. If E *failed*, the same argument shows that M *failed*.

(3) If E had had more practice in observation, he would then agree with M and with Nafe's other *O*s.

It is true that M had had more training in introspective psychology at the start of the experiment than E had at its close. M observed throughout a major experiment quite like the present one; E, on the other hand, was serving his first term in this type of experiment.

It is impossible to predict what the situation might be after 800 reports; but after 80 it is clear that E's reports are not in any way approaching M's. E's type of report is fairly stable and I guess that further repetition would give only further habituation to a type of report in the case of E and also in the case of M.

Fundamentally I object to taking M's reports as a standard and asking 'Why did E *fail* to report as M did?' It is equally straightforward to ask, 'How much longer would the experiment have to continue for M's reports to 'fall in line' with E's?' I think it is just as likely that further practice would modify M's report as E's.

If it be true, as I am claiming, that the theoretical views of an *O* are essential conditions of his report, then a change of 'heart' rather than further practice is needed to bring about uniformity in a group. Why should we assume that one report is better than another?

OBSERVER N

At the close of three of the series N made comments which are reported below as 16A, 48A, and 56A.

Reports. (7—caryophyllorum) That was nice. It was a spicy smell. I'd say it was *P*. It would be more *P* if it hadn't been quite so intense. Didn't bring up any gustatory imagery.

(9—carbon disulphide) That was *U*. It was half-way between nauseating and sickening. It had something in it that smelled a little bit like ether. It was moderately intense. I just now get an after-image of it. I still have it faintly.

(16A) The ones you gave me are chiefly *U*. I believe you gave me only *U* ones in the series. Most of them were very *U* and especially the ones like decaying food matter. I got after-images of the most intensely *U* ones and not of the mild ones.

(23—pyridine) It's the worst one I've had so far. Very *U*. I get a very strong after-image of it. I still have the after-image and I still have the taste in my mouth. It is a nauseating odor, very much so. It left a terribly bitter taste in my mouth. Now the image of the smell itself has left but I still get the taste.

(29—C, E, G, C′) First two notes were *P*. The third note made a noise, so did the fourth. Towards the end of the vibration the whole became *U*. No after-image and no organic stimulation. Each note in itself was *P* but when the last note was combined with the others it gave the *U*.

(37—vinegar) That was *U*. It had a very sharp taste. It gave a peculiar *U* sensation in the stomach. I still taste it. It has lost its sharpness but it is still *U*.

(41—oil mace) That was slightly *U*. I guess it was *U*. If it had been a little milder, it would have been *P*. It was a strong spicy smell.

(48A) I haven't yet found *U* as organic except in one or two instances—in a smell and in one taste there were organics. I did get organic sensations in drawing my hand away in the pain. I drew away from the sound a few days ago.

(52—colored band) I'd call that *P*. I like the brighter colors toward the center best. I thought most of the colors were good colors.

(56A) There is some organic reaction with the *U*. Odors make you pull your head aside or shudder. Mild stimulations don't seem to give the organic reactions that the strong ones do, or if there is any organic reaction, it is so slight that I didn't notice it.

(58—phenol) That was mildly *U*. It smelled like strong carbonated vaseline and it brought up associations about the sick room.

(70—hot steel) That was *U*. It was hot and it made me unconsciously try to draw my hand away. It left a pain sensation. I felt heat while the heated object was pressed on it. Now I feel pain.

(77—sarsaparilla) That's very *U*. It gives me a slight shuddering feeling. Taste is still in my mouth.

Questionary. After the experiment N was asked the following questions. A complete record of questions and answers is given.

Q: Are you familiar with a study of feeling by Nafe?

A: No.

Q: Are you acquainted with Titchener's touch pyramid?

A: No.

Q: Will you tell me briefly your theoretical views about *P* and *U*.

A: My opinion is entirely unbiased. I don't think I had any views at the start and I don't have any views now. I don't think *P* and *U* are much connected with organic sensation. It is only in extreme *U* that organics appear. I haven't had much *P* so I haven't formed any view about that.

Q: Have you read any psychological papers on feeling?

A: No.

Q: What courses in psychology have you taken?

A: I have had Psychology I (introduction) and II (general), and I am now taking III (elementary laboratory) and VII (mental imagery). Feeling has not been stressed in these courses and I think I am really unbiased.

Q: What do you think the experiment is about ?

A: I think you are trying to find out whether the sensations which you give (odors, etc.) are connected in any way with bodily sensations.

Q: What do you think about the instructions ?

A: I think they are all right. I think they are clear. I wasn't quite sure of how much you wanted in the introspection.

Q: What did you understand your task of observation to be ?

A: First whether it was *P* or *U* and to describe the feeling if I could and to tell whether it was connected with organic sensations. I gave that about after-images but I don't know whether you wanted it or not.

Q: Are there any criticisms of the experiment ?

A: No. Only one thing. I don't see where the *P* comes in. I think it was an experiment chiefly on *U*.

Q: Is there anything further you should like to add ?

A: Nothing now.

Discussion. The terms 'bright pressure,' 'dull pressure' or their equivalents are not to be found in the reports of N. There is no hint that *P* and *U are* pressures of any kind, or that they are associated in any way with pressures.

N makes use of such psychological terms as she has acquired from her courses of study. The frequent reference to after-images should be related to the fact that N, during the experiment, was registered in a course upon mental imagery. In reports (not printed) describing auditory experiences N uses such words as 'partial tones,' 'volume,' 'beats,' 'brightness quality,' 'intensity,' etc. These descriptive categories are most certainly derived from Psychology I and III. Also in reports of smell qualities and associations the influence of elementary courses in psychology is apparent. Had N been acquainted with Titchener's touch pyramid[32] as were *all* of Nafe's *O*s,[33] the terms 'bright pressure,' and 'dull pressure' might have appeared in N's reports.

The reports of E and N are similar in that both are relatively brief and also contain a good deal of material beside the point of the experiment. Both are naïve. M's reports, on the other hand, are longer, possibly more to the point of the experiment, and certainly more sophisticated than those of E and N.

Summary

Nafe's experiment was repeated using one of his original *O*s. The present reports of this *O* duplicate her former efforts. A second *O*, who had had a radically different type of psychological education, failed completely to confirm Nafe's result but gave reports consistent with his theoretical views upon *P* and *U*. A third *O*, relatively unsophisticated in psychology, also failed to confirm Nafe's result; her records show the dependence of report upon psychological vocabulary.

Practice in the sense of mere repetition of observation under controlled conditions is not sufficient to duplicate Nafe's result. Practice was limited to 80 reports, 8 a day for 10 days, and at the close of the experiment there is no hint that further practice would change the result. It is just as likely that M would change her type of report with further practice as that E and N would modify theirs.

A 'trained' *O* is not necessarily one practiced in psychological observation. The important factor in training is the psychological education or indoctrination to which the *O* has been exposed. The terms of description which an *O* has acquired, the psychological facts he knows, and his theoretical views very largely determine the type of report.

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[32] E. B. Titchener, this Journal, 31, 1920, 213.

[33] *Op. cit.*, foot-note 517 f.

IV. The Logic of Affective Psychology[34]

The confusion and contradiction found to-day within affective psychology are notorious. Upon the most fundamental matters there is little agreement among psychologists.

If one consider the problem of the basic nature of affective processes, one finds at least three contending views. (1) There is the traditional view of Titchener and others that *P* and *U* are unique, non-sensory, non-clear mental processes which vary in duration, quality and intensity. This view was tacitly accepted by myself in 1917-18 when working on the affective processes at Cornell, and the view seemed to work very well in the laboratory.[35] (2) There is the view expressed by Yokoyama in 1921 that *P* and *U* are statable as meanings.[36] Two of Yokoyama's *O*s, confessing to be biased in favor of this view, confirm it in their observations. All of the *O*s may report *P* and *U* as meanings. (3) There is a view most recently stated by Nafe that the affective qualities under direct observation are modes of pressure: *P* is a bright pressure and *U* is a dull pressure.[37] This is a special variety of the sensational views of affection.

In less fundamental matters a similar state of turmoil prevails. I have previously pointed out that the uncertainty about the localization of affective processes rests upon ambiguity of the terms 'feeling' and 'localize.'[38] Confusion about the antagonism and coexistence of *P* and *U* can be traced to the common use of ill-defined terms such as 'cognitive awareness,' 'meaning,' 'affective process,' etc.

In Section III it was demonstrated that experimental data in affective psychology depend largely upon the theoretical views of the *O*.[39] This consideration in itself brings to clear light the source of the present confusion. How can we hope to *discover* anything in affective psychology if the data from which we start are so largely *made* by our logic? I will return to this question later.

At this point let me pause to give an analogy. When visiting Yale University a friend once demonstrated a dog that had been trained to lie down as if dead whenever the name 'Harvard' was pronounced; if one should say to the dog 'What would you rather do than to go to *Harvard*?'

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[34]The material of this paper was presented to the American Psychological Association at Ithaca, N. Y., December, 1925.

[35]P. T. Young, An experimental study of mixed feelings, this Journal, 29, 1918, 237-271.

[36]M. Yokoyama, The nature of the affective judgment in the method of paired comparison, this Journal, 32, 1921, 358, 368.

[37]J. P. Nafe, An experimental study of the affective qualities, this Journal, 35, 1924, 507-544.

[38]This Journal, 38, 1927, 180. "As to the meaning of the term (feeling), it is plain that further definition is requisite for a word that may mean (*a*) a touch, as feeling of roughness; (*b*) an organic sensation, as feeling of hunger; (*c*) an emotion, as feeling of anger; (*d*) feeling proper, as pleasure or pain." J. Ward in *Encyclopaedia Britannica*, article 'Psychology,' section 5.

[39]I have long questioned the emphasis ordinarily given to 'training.' It is customary to give the academic training of the *O*s, their degrees, and professional status, and frequently their psychological biases. See, for example, Yokoyama, *op. cit.*, 358.

immediately the dog would drop down as if dead. Now since one dog can learn a trick, it is probable that a group of dogs could be trained to react similarly and uniformly to the spoken word of 'Harvard.' Doubtless also another group of dogs at Princeton University could be trained to sit up uniformly and bark whenever the word 'Harvard' be pronounced. Now I ask: Which is the true, the genuine, the *scientific* type of behavior? Which type of behavior most accurately describes or expresses the ultimate essence of existence?

So in affective psychology one kind of training and one set of laboratory conditions may lead to a roughly uniform type of verbal report; but another type of training may yield an incompatible, contradictory result. Which type of report is true, and which is false? My own answer is that both results must be regarded as a product of conditions. The illustration from animal training is more than an analogy; for biologically regarded the human observer is also an animal and his training in speech is quite comparable to the training of the dog.

I am aware that some of my readers will object. The report of a trained *O* in a psychological experiment is not wholly the product of training. Surely there are existential mental processes, sensory or other, which at least to some extent determine the *O*'s report. Surely the introspective report is a direct expression of the *O*'s experience. It is 'symptomatic' of some phenomenal datum in the *O*'s field.

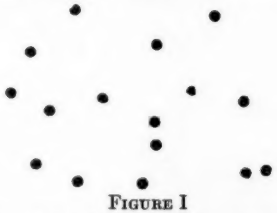

FIGURE I

I admit the justice of the objection and so proceed to a second analogy Let the eye move at random over the group of dots in Fig. I. Into what patterns and groupings do they fall? Some are grouped by threes; others by fours; others in more complex patterns. With proper instruction one may see large and small groupings, regular and irregular, and perhaps no groupings at all. Seemingly an indefinite number of shapes and patterns may be perceived.

So in affective psychology the so-called facts depend upon highly variable and little understood conditions. Trouble begins when we attempt to say which observation is true and which is false. The determination of the truth or falsity of an observation appears to be about as futile as to argue whether the dots are truly grouped in threes or fours, in this way or in that way.

Some of the conditions which determine the report in affective psychology are (a) the *O*'s education in psychology which includes the kind and amount of his information, (b) the *O*'s bias determined in part by his theoretical reflections, (c) the *O*'s understanding of words and his habits of speech, and (d) the suggestions which happen to reach him from various sources.

These factors, except possibly suggestion, may be grouped together under the term 'training.' Introspective investigators of feeling usually recognize the importance of training. Train-

ing is admittedly important; but perhaps it is too important. It is more important than is generally realized. It is so important that through training one can apparently demonstrate in affective psychology almost anything one likes. I will guarantee to train a group of unsophisticated *O*s to report uniformly that *P* and *U* are non-sensory affective processes, or meanings, or pressures, or bodily reactions, or something else. I will guarantee to train a group of naïve *O*s to localize *P* and *U* or to declare that these processes are non-localizable, to report mixed feelings or to declare that mixed feelings do not exist, to describe qualitative diversity within the qualities of *P* and *U* or to report lack of qualitative diversity, and so on indefinitely. The only requirement I make is that my *O*s be young and plastic. Occasionally I might have to discard the 'bad' reports of a recalcitrant *O*.

It is my belief that mere practice within a group of *O*s is not sufficient to guarantee a uniform result in affective psychology. Uniformity can be gained in a group through controlling the education of the group; but through the control of training one can demonstrate, with apparently equal validity, results which are logically incompatible.

The futility of the present situation is rather discomforting, but let us ask in a positive way: "What can be done about it?"

(1) I repeat that the attempt to determine what is true and what is false, along present lines, is futile unless we are ready to accept simultaneously a mass of logically opposed, incompatible, mutually contradictory propositions as being equally true. Unless, to take an analogy, we are ready to argue that the dots are truly grouped in this way and not in that way; unless we are willing to insist that this particular trained dog has the only virtuous, significant, and genuine type of performance. Almost any doctrine can find experimental support.

(2) The number of observational tricks which an *O* in affective psychology can be taught is seemingly indefinite. Certain tricks, however, are incompatible. A dog may be trained to bark or to lie down quietly at the spoken word; but he cannot be trained to bark and to keep quiet at the same time. So an *O* may be trained to report *P* and *U* as bare, non-clear process, or he can learn the pressure trick, or the meaning trick—but he cannot perform logically opposed tricks simultaneously. How many observational tricks are possible? The number is seemingly indefinite.

(3) The present situation can be cleared up only by the frank recognition of the complete relativity of so-called fact to the logical system which is the product of training. It is important to find common ground which is independent of training. In

the search for data which are independent of training I see hope; not in further training of trick *Os*.

A worth-while question is not whether this or that trick observation is true or false, but rather, "How did the creature get that way?" This is a genetic problem and it demands that we begin at the beginning.

Now there are data in affective psychology which are relatively independent of training and bias. Such types of performance as vomiting, the movements associated with bodily injury, the restless movements accompanying starvation and internal diseases, groaning, crying, etc., may be considered collectively as examples of disliking which are relatively free from the theoretical opinions of the afflicted person. On the other hand, the sight and smell of well-cooked food and the eating of the same when hungry, the specific sex responses, and probably other basic types of performance may be taken collectively as examples of liking which are relatively independent of psychological training. In the study of the basic affective reactions which are relatively independent of training I can see a way out of the present difficulties.

At present we are attempting to build a roof before we have a house.

V. The Framework of Psychology

Datum and Logic. The starting point for the natural sciences is a simple and direct acceptance of phenomena just as they are given to some observer. The individual phenomenon is observed by someone at a particular time and place. As examples of phenomenal data consider: a stone, a star, a ghost image, my living body, the color red. Reflection upon phenomena leads to the construction of logical systems, for example: the hypothesis of the electron, the theory of relativity, the idea of God.

The relation between datum and logic is exceedingly close. The logical construction of an observer in part determines the nature of the datum, for example: whether it be a ghost-to-be-feared or simply an interesting after-image. Many of our beliefs have never been logically formulated; they rest, perhaps, upon an irrational foundation; they are not the cold, clear, logical outcome of reflection upon data.

In the natural sciences the aim is to build up logical constructions, in words or in other symbols, which rest upon observed data. *Observation* is the key word to the sciences. Along with observation the man of science reflects and reflection leads to the construction of general propositions, the formulation of hypotheses and theories, the stating of problems.

The relation between datum and logic is circular, as shown in Fig. II. A logical system is built upon observed data. Reflection upon the data, or the discovery of new data, shows the weakness of a system and this leads to further observation. The logical structures of any field of learning are continually being modified to keep them adequate to the facts of observation. The facts in part make the logical construction what it is. The logical construction is a condition for the observation of new facts. The truth of a logical system is relative, pragmatic.

Objectivity. Normally most phenomenal data are taken to be objects which exist externally to the observer, which are spread round about, and which are relatively independent of the observer. In some cases—hallucinatory voices, visions, dream objects, after-images, entoptic phenomena, and the like—this objective character is commonly taken to be false or

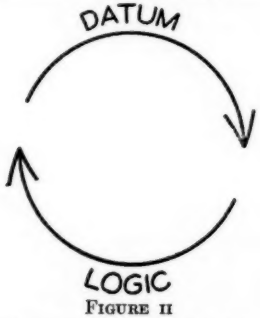

FIGURE II

inadequate. Primitive man may take his dream experience objectively and later find that to do so leads only to confusion. Again, certain phases of the phenomenal order—feelings and emotions, ideas, desires, etc.—appear to be relatively lacking in objectivity; they are usually referred to the subject of experience, or regarded as mere experiences.

These considerations may be generalized by saying that objectivity is a characteristic of some phenomena but not of all, and that the character of objectivity is sometimes taken to be valid and sometimes invalid.

Where Objectivity is Valid. In such studies as astronomy, physics, chemistry and most of zoölogy objectivity is tacitly accepted as a valid characteristic of phenomena. The objects observed are taken as if they existed externally to the observer and independently of him. Furthermore, these objects are frequently believed to exist in a real order which is independent of the given, or phenomenal, order of existence.[40] Practically we all believe that the inference of a real order is valid.

[40]When once we realize that the real object is an inference and seek to understand it through logical analysis we are left with a bare object of perception and a point of view taken up towards the object. We are reduced to datum and logical system.

In contemporary American psychology there is an attempt to limit psychology to the study of behavior regarded physically. It should be realized that in accepting the logical framework of the physical sciences one has limited psychology to the sphere within which objectivity is commonly taken to be adequate. This limitation is methodologically sound. The study of animal movement is undoubtedly a genuine and important part of zoölogy. The physical behaviorist, however, has unduly limited the scope of psychology and has ignored the mass of data which historically have been regarded as constituting the subject-matter of psychology.

Phenomenal Relativity. All phenomena, taken as existences, are found to be relative to one object—my body and more particularly my neural structures. By 'my' body I mean in general the body of any observing, experiencing, human individual. The relativity is such that bodily changes in me modify or remove the phenomena. The age-old experiment of opening and closing the eyes is instructive;[41] the visual world with its objects appears and disappears at the wink of an eye. The eye is an all-important factor in the existence of visual phenomena. If I change the position of my eye, the entire structure of the apparent physical world shifts.

During dreamless sleep, anaesthesia, injury to the nervous system, all phenomena—so far as I am aware—cease. Here one finds phenomenal relativity of the all-or-none variety.

Conscious and Unconscious Conditions. I assume that sometimes I am unconscious. I never experience the unconscious condition for to experience it would be to be conscious. The unconscious condition is thus not datum but inference.

Whenever there is present some phenomenon, some datum to be observed, studied, talked about, I assume that I am conscious. My criterion for the conscious condition is thus the presence of some phenomenal datum that may be observed. This is a psychological rather than a physiological criterion, by which I mean that the great bulk of phenomena are not given as physiological processes. The bodily processes themselves constitute a small part of the total phenomenal data.

It may be said that awareness itself is ultimate and that no criterion is needed. One may simply assert 'I am now conscious' and let matters rest there without further evidence for the statement.

Also, in the sphere where objectivity is valid, the conscious condition may be regarded as the total conditions found to be essential to the existence of a phenomenal field. For example,

[41]Since the real world is generally believed to be substantial, it is very easy to regard the phenomenal world as itself a substance; but the concept of mass, for example, is not applicable to the appearance of objects. If an individual were weighed on a sensitive scale with eyes alternatively open and closed, no difference in weight—with the refinement of technique that is possible today—would be expected. Even a behaviorist, observing a white rat running a maze, would weigh the same when his eyes were closed and the phenomenal rat gone.

I may lose a limb or an appendix without permanently losing consciousness; but, so far as I know, the destruction of the nervous system is associated with the unconscious condition. Hence the immediate conditions for the existence of all phenomenal data reside in the nervous structures, although the phenomenal structures themselves are also dependent upon extra-neural conditions. The differences between dreamless sleep and normal waking life are matters for experimental investigation rather than for speculation.

Assumption of Phenomenal Fields. Where communication is possible, as among men, it is assumed that persons other than one's self are conscious. How about animals?

I walk down the street and watch a dog on the porch across the street. As I walk the dog's head follows my movements whether I walk fast or slowly, forward or backward. I assume that the dog is conscious in the sense that he too has a phenomenal field more or less like my own. He too sees objects before him and moves in relation to those objects. My assumption rests upon the observed movements of his head and neck, and also upon the study of his eyes and nervous system. The dog's visual field, which I assume to exist, is not to be confused with his movements (behavior). The dog's field is thought to be similar to my own; it includes objects seen and heard as well as the experiences of bodily movement. The dog's field, which I assume, is not to be confused with my field. I may believe that one and the same real object determines both fields, but there are always two perspectives—the dog's and mine—and they cannot easily be made to coincide.

Whether the assumption that the dog is conscious—in the same sense that I am conscious—is of value in interpreting the dog's behavior, and how the assumption may be utilized in the study of behavior, are problems for the psychologist. I am content to point out now that the assumption of a field is distinctly a psychological assumption, by which I mean that the consistent physical behaviorist does not make the assumption and cannot make it without departing from his physical postulates. The assumption of the field, however, is not made in violation of any physical postulate, so far as I can see; but it is made over and above the physical postulates, *i.e.* in addition to them. If the psychological facts demanded an assumption wholly contradictory to some physical postulate, I should not hesitate a minute in making that assumption.

The Principle of Full Adequacy. In common with other sciences psychology studies the inter-relations of phenomena. The student of behavior studies the movements of the white rat in relation to the structure of the maze and to other controlled conditions. The student of sensory datum studies qualitative and other characteristics of experience in relation to processes in the receptor organs and in the nervous system.

Along with the study of the inter-relations of certain phenomena there is frequently the need of making a guess, *i.e.*

supplying an hypothesis. In constructing an hypothesis the psychologist looks to the living body and asks: What must be assumed about the living organism to account for the phenomena now in question? What is fully adequate?

As a matter of method the psychologist may assume, perhaps, a real mind[42] as the basis of mental processes. Whether the assumption of a real mind is necessary or not remains to be seen. Ultimately the characteristics of the real mind may be found to be identical with the characteristics of the nervous system; so that two postulational systems—a mental and a physiological—may not be necessary.

As a practical faith the psychologist may assume that in the real world there are conditions *fully adequate* to account for every fact and phase of the phenomenal order. What must be assumed depends upon the facts, and the road to the facts leads to the laboratory.

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[42]Some of the characteristics of the real mind as it is popularly known are these: experiences leave impressions on the mind which are stored unconsciously and which later determine behavior and experience; the mind may be *set* as when one says 'I have made up my mind;' there are sometimes conflicts in the mind, especially between two possible courses of behavior; a person may be of two minds or determinations; a person may lose his mind, in which case his mind may disintegrate; the mind is involved in conscious thinking, remembering, desiring, liking and in other functions of the organism; the mind is free and capable of self-determination; the mind is dynamic; the mind travels from place to place with the body; the mind wanders a thousand miles from the body.

In so far as we leave out of account the facts of the field of consciousness the remaining characteristics of the popular 'mind' are known to be characteristics of the nervous system. The nervous system is modified through reaction; it retains; it may be set; it travels with the rest of the body, etc.

FIGURE AND GROUND IN THE VISUAL PERCEPTION OF FORM*

By Ernest Glen Wever, University of California

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New points of view in psychology have arisen most frequently in the field of perception. The development of *Akt-psychologie*, of the school of *Gestaltqualität*, and, most recently, of the psychology of *Gestalt*, has in each case been due to a dissatisfaction with the traditional methods of dealing with the problem of perception. In this problem the most insistent phase, and one which is crucial to a solution (as is shown by its prominence in the theory of the last two movements just mentioned) is that of the visual perception of form.

The greatest contribution to the theory of visual form-perception is the study by Edgar Rubin, in which the fundamental type of form-experience was found to consist of a figure standing upon a ground. The central feature of the perceptual experience is the figure; the marginal or recessive portion is the ground. One member of a whole cannot be considered alone; it can exist only in its reciprocal relation to the other member. This last point, only suggested by Rubin, has more recently been expanded as a primary principle of the *Gestalt* approach to perceptual theory. The figure-ground experience is the fundamental type of perception: every perception is a totality, a *Gestalt*, in which appear two components, the highly structured part, which is the figure, and the vague, undifferentiated, unformed portion, which is the ground or level from which the figure emerges.

In its rise in this theory of perception the *Gestalt* psychology has come into direct conflict with traditional systems. Perception in the older theory, to which Wundt gave authority, was set over against the (sensa-

*From the Harvard Psychological Laboratory.

tional, imaginal) elements as more complex, and as analyzable into these constitutive elements. As the critics of the sensationistic position point out, there are obvious difficulties to such a scheme, difficulties so great that at no time has a psychology of perception been attempted within the boundaries as described. Other affairs—attention, association, act, form-quality, creative synthesis, attitude—are called upon in attempts to complete the psychological picture. Some of these affairs constitute the residuum after analysis into sensational elements; others are agencies by which is explained the binding relationships between the elements revealed by the analysis.

There are here, then, two views of mental structure—views widely different, and not to be confused because of the disparate use of the term 'structure.' In the one case mind is thought of as a structure whose properties are a function of the concurrence of certain primary elements; in the other case the situation is reversed, the primary affair is the totality, the *Gestalt*, and the properties of the parts, if indeed parts appear, are functions of the whole. The phenomenon of figure and ground may be used to illustrate this difference: in the traditional theory the experience might be explained as due to the coincidence of certain elementary experiences—a visual sensation (or group of sensations) of a particular quality, extent, duration, and clearness, and another visual sensation (or group of sensations) of somewhat different attributive properties; in the *Gestalt* theory, on the other hand, the experience is a whole, a *Gestalt*, in which one may find the components, figure and ground.

In the first case the method is one of cross-sectional analysis, by which the complex (perception) is resolved into its parts (sensations). In the second case such analysis is denied; the perception is not complex in the sense of being a sum of parts, but is a unitary configuration. The method of phenomenology is substituted for the analytical procedure, and the wholes (*Gestalten*) are taken as they come, in their own right, and described, not in terms of any arbitrary set of elements, but in any manner that seems adequate to the practical situation.

A great deal of this criticism of traditional methodology is well-founded. In fact, the principal objections made by the *Gestalt*-psychologists had been made previously, and were fairly well recognized. A question may be raised, however, about the matter of analysis. If the traditional method of analysis is invalid, is any analysis at all to be permitted? If so, what may its nature be? Apparently, a fundamental sort of analysis is allowed in the recognition of figural and groundal components in visual form-perception. Is this the end of analysis, or by appropriate procedure may one go farther? May there be discovered certain descriptive features which will identify the figure-ground experience? A solution of the problem for this fundamental, simplest type of *Gestalt* should have general pertinence to theory of structure and to analytical method.

That the figure-ground experience does, at some times at least, possess certain descriptive features was established by Rubin. One of these features is the thing-character of the figural component. The figure stands forth in the experience with object-character, the ground is of the nature of substance. The figure is said to be more highly "structured;" the greater the degree of "structuration," the "better" the experience. The nature of this matter of 'structuration' is obscure and will bear investigation. 'Structuration' might be regarded along the lines of traditional theorizing as a binding of the elements, as a process in which an act of the subject brings about the intimate grouping of the parts, or again, and more simply, as a process of accrual by which parts are added to a present core, giving rise to a new group (structure).

This raises in concrete form a very old problem, the problem of meaning. Is the 'structure' meaningful? Is the thing-character of the figure a mean-

ing-designation? In a careful investigation of the figure-ground phenomenon one may hope for some light upon this general problem.

This whole problem is fundamental to the science. The problem of perception involves the basic problem of analytical method, and this in turn a theory of the nature of mind. A theory of mind as unitary structure raises the question of a general, fundamental analysis analogous to the figure-ground analysis already conceded. This is the general setting of the present problem. The specific setting is the question of the visual perception of form, and its keystone position in contemporary theory. The

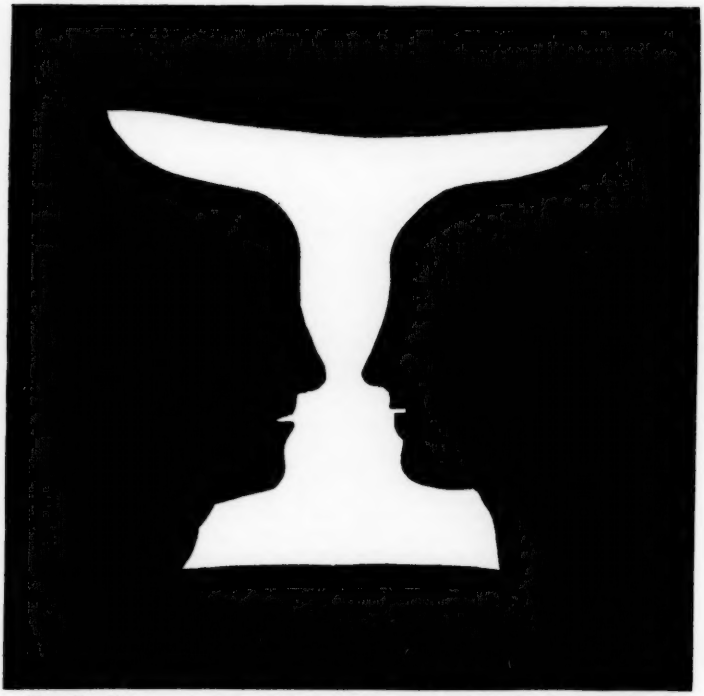

FIG. I. GOBLET-FIGURE OF RUBIN. Reprinted from E. Rubin, *Visuell wahrgenommene Figuren*, 1921.

general suggestion of method issues from Rubin's recent work on figure and ground, and the hint of analytical relationships. The specific suggestion of attack is drawn from Rubin's procedure with ambiguous (reversible) stimuli, a procedure which is bolstered up by observations on figure-ground reversibility in previous work on visual form-perception.

HISTORY OF FIGURE-GROUND PHENOMENA

The distinction between figure and background in visual form-perceptions is one of long standing. It has been recognized, in portraiture for

example, that the field or setting in which the figure occurs is an important determinant of the artistic value of the whole. The figure-ground relationship is most forcibly demonstrated in those instances in which the fields are reversible, where what formerly was figure may now be perceived as ground, and conversely.[1] This ambivalency of fields was not unknown in ancient art. It was a frequent motif in Egyptian, Arabian, and Indian design. The familiar goblet-figure of Rubin's (Fig. 1) was taken from an old vase. The picture-puzzle, in which a profile is concealed in the detail, is an ancient device.[2]

Psychological interest in the ambiguity of visual fields is derived in part from the study of optical illusions, particularly illusions of reversible perspective.[3] Helmholtz observed in 1867 that the Zöllner pattern, which usually appears as black stripes on a white ground, may also be perceived as white feathery branches lying on a black ground. This he explained as due to eye-fixation: the change can be effected by fixating firmly the white stripes.[4] Hering (1879) described an illusion in which light shining through a colored window is taken for colored spots.[5] In the course of a discussion of depth and perspective, Mach (1886) mentioned the reversal of visual fields in ornamental design.[6] Jastrow (1901) illustrated a number of reversible illusions, and gave a casual account of the conditions underlying reversal. The changes he ascribed to what "is in the mind's eye" and to familiarity, prepossession, and other subjective factors.[7]

Schumann experimentally attacked the problem of visual form.[8] In his work on grouping he found that when a number of parallel vertical lines are seen in pairs the white stripe between the lines forms a simple whole (the 'figure') and protrudes in consciousness, while the white surface between the groups ('ground') retreats and appears quite different. Not only does the lattice protrude, but the plane of the paper withdraws spatially.[9] The same holds for the checker-board designs. The conditions for the formation of a group ('figure') are spacing of the elements and volition. When the elements are similarly arranged volition operates, and the group changes readily. Spacing is sufficient for the formation of a group without regard to volition.

Schumann regarded visual form as a combination of elements into a unit or group. He argued against the theory of form developed by the school of *Gestaltqualität*, in which a new element, the form-quality, is added to explain the properties of the whole.[10] Schumann's experimental findings were verified by Zigler, who used checker-board patterns, incompleted line drawings, and other material similar to Schumann's.[11] He found that the marks differentiating a group may belong to the intervening spaces, the elements, and tied imagery. The spaces may become broader, or lighter, or darker. The elements may become blacker, clearer, more extended, and standing out in perspective.[12] Zigler agreed with Schumann in discarding

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[1]The terminology follows E. Rubin, *Visuell wahrgenommene Figuren*, 1921, vii, 5. If Fig. 1 is perceived as a vase or 'goblet' the white surface is "figure," while the black surface is the "ground" upon which the goblet is perceived; "field" is a neutral term applicable to both surfaces alike. The term 'stimulus' will be employed as a designation of two fields such as Fig. 1, irrespective of the experience which may arise.

[2]Lillien J. Martin's "King Louis XVI" device, taken from an old coin: Uber die Abhängigkeit visueller Vorstellungsbilder vom Denken, *Zsch. f. Psychol.*, 70, 1914-5, 213.

[3]See J. E. W. Wallin, *Optical Illusions of Reversible Perspective*, 1905.

[4]H. von Helmholtz, *Handbuch der physiologischen Optik*, 1867, 566 f.

[5]Hermann's *Handbuch der Physiologie*, 1879, III, 574.

[6]Ernst Mach, *Analyse der Empfindungen*, 1886, 89 f.

[7]J. Jastrow, *Fact and Fable in Psychology*, 1901, 294 f.

[8]F. Schumann, Beitrage zur Analyse der Gesichtswahrnehmungen, I, *Zsch. f. Psychol.*, 23, 1900, 1-32; II, 24, 1900, 1-33; III, 30, 1902, 241-291, 321-339.

[9]*Op. cit.*, I, 6-9.

[10]Cf. the discussion by M. Bentley, The psychology of mental arrangement, this JOURNAL, 13, 1902, 269-293.

[11]M. J. Zigler, An experimental study of visual form, this JOURNAL, 31, 1920, 273-300

[12]There are also changes in the contour lines. See below, p. 205.

the *Gestaltqualität*, and accounted for the perception of form as an attributive pattern established under a particular attitude, and correlated with a definite form-meaning.[13]

The ambiguous character of visual fields forced itself to the notice of Rubin, since it affected grossly the results of an experiment upon the recognition of previously exposed stimuli. He used irregularly shaped green fields on a large black screen, and found that frequently the black instead of the green field was perceived as figure. Rubin's results are briefly summarized:

(1) If an ambiguous stimulus has previously been experienced with a given field as figure, the same field is more likely to be figure on a second presentation. This is the *figural after-effect*.[14]

(2) The recognition of an ambiguous stimulus is dependent upon the experiencing as figure of the same field as in previous presentation.[15]

(3) The figure and the ground are different experiential objects. (a) The figure has form; the ground in a sense has no form. This form seems to proceed from the contour.[16] The contour may so little limit the ground that it appears as extending uninterruptedly behind the figure. (b) The figure has thing-character; the ground is of the nature of substance.[17] (c) The figure has 'surface-color;' the ground is more likely 'film,' less dense, more primitive. Memory-color operates more upon a field when it is experienced as figure than when it is experienced as ground.[18] Irregularities of lighting are less striking when the field is figure; shadows are levelled off.[19] (d) The figure usually is localized in front of the ground.[20] (e) A field when figure is more *eindringlich*. It is named sooner. It is better remembered. Its color is more impressive. Differences in two stimuli are more readily reported when the compared fields are both figures than when both are grounds.[21] The figure is more likely to have connected with it various meanings, feelings, and esthetic values.[22]

Since Rubin's study there have been several investigations of phases of the figure-ground phenomenon. Von Hornbostel, 1922, in an investigation of reversible perspective, characterized the experience of inversion as a change of convex into concave, and concave into convex. The convex has figural properties, the concave the character of ground. It is in general more difficult to invert the convex than the concave.[23] Koffka (1922) found that in retinal rivalry a field plays a different rôle as figure than as ground.[24] Hartmann, in the investigation of fusion, found that flicker disappears for a white field sooner if the field is perceived as ground than if it is perceived as figure.[25] Gelb and Granit (1923) found that the color-limen has a larger value when the field is perceived as figure than when it is perceived as ground.[26]

Rubin's success in isolating the properties of figure and ground was due in no small measure to his use of completely reversible stimuli. An irregular green field within a dark area may be experienced at one time as green figure on black ground, at another as a dark surface with an irregular opening beyond which lies the green background; the change from one phase to the other is attended by a striking change in the object-character of the experience. It is interesting to add, however, that with certain other

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[13] *Op. cit.*, 299 f. [14] Rubin, *op. cit.*, 6-21.
[15] *Ibid.*, 21-29. [16] *Ibid.*, 36 ff. [17] *Ibid.*, 46-51.
[18] David Katz, Die Erscheinungsweisen der Farben, *Zsch. f. Psychol., Erg. Bd.*, 7, 1911, esp. 214 ff.
[19] Rubin, *op. cit.*, 52-59. [20] *Ibid.* [21] *Ibid.*, 67ff, 70 f, 41 f. [22] *Ibid.*, 73-79.
[23] E. M. von Hornbostel, Ueber optische Inversion, *Psychol. Forsch.*, 1, 1921, 154.
[24] K. Koffka, Die Prävalenz der Figur, *ibid.*, 2, 1922, 147 f.
[25] L. Hartmann, Neue Verschmelzungsprobleme, *ibid.*, 3, 1923, 363 f.
[26] Die Bedeutung von "Figur" und "Grund" für die Farbenschwelle, *Zsch. f. Psychol.*, 93, 1923, 83-118; esp. 92-3.

stimuli, such as the identical-field patterns often encountered in ornamental design, one may observe a complete reversal of fields without such change of object-character.[27] On the other hand, it is possible to observe a change of object-character without concomitant reversal of fields: in the familiar "Brain and Babies" drawing of Gudden's[28] one sees in the first case the convolutions of a human brain, in the second a group of sprawling infants, but in each instance the ground is the white surface of the paper.[29] Similar to this is the experience in illusions of reversible perspective; to take the well-known instance, a stairway viewed from above may suddenly reverse, and become a stairway seen from below. Here again the ground is altered little if at all, but there is a striking change in the perspective of the figure. A third phenomenon of this class is one in which the change is of grouping, illustrated by Schumann's dot-diagrams, which may be observed as horizontal rows or as vertical columns, while in each case the ground is the white surface. The ground here may change slightly, but it does not reverse. Intermediate between these affairs in which the figure may undergo alteration of object-character, perspective, or grouping, and the experiences in which reversal of figure-ground fields may be complete, stands a class of form-experience in which some limited portion of the ground may suddenly come out as figure, while the previous figure more or less recedes. This is best illustrated by the familiar 'picture-puzzles;' a case already referred to[30] is the "King Louis XVI" device. The primary figure is a large vase on a stone wall; then one may see the profiles of King Louis and his family: two outlined by the vase, and one on each side hidden in the branches. At no time does the vase disappear, though its prominence may somewhat diminish. The reversal here is but partial.

As this discussion shows, not an inconsiderable portion of our visual experiences belongs to the class of equivocal forms, a class to which may conveniently be applied the term "polyfigurations," as used by Werner in a discussion of visual *Gestalten*.[31] The correlative class in Werner's terminology, the "monofigurations," comprizes the larger remainder of our visual world. Monofigurations may be stable, in which case they are little subject to alteration, or instable, in which instance they may undergo limited change. The face of a friend is a stable monofiguration; it is insistent upon being figure and not ground.[32] Familiarity (figural after-effect), centrality of position, small size, and movement are factors that make for stability: the last three are optimally shown in the 'falling star:' it is a small, luminous, rapidly-moving line, seen against the dim, unformed sky; it takes one unawares and lasts but a moment—it quite precludes reversal.

Instable monofigurations are complex visual affairs in which parts may at different times be more or less dominant in the experience, but the change is not of great magnitude. During the examination of a portrait, or a simple tapestry, now one feature, now another, may be prominent, but the general character of the experience remains constant.

It is apparent from this survey that changes of figural and groundal status are not uncommon in phenomena of visual form. The suggestion appears, moreover, that in the figure-ground relationship lies a unifying

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[27]See the pattern illustrated by Mach, *Analyse der Empfindungen*, 1886, 89.

[28]Illustrated by E. B. Titchener, *Textbook of Psychology*, 1910, 278.

[29]Cf. also the J. Jastrow "Duck-Rabbit" figure, *Fact and Fable in Psychology*, 1901, 295.

[30]Note 2, *supra*.

[31]Heintz Werner, Rhythmik, eine mehrwertige Gestaltenverkettung, *Zsch. f. Psychol.*, 82, 1919, 198-218.

[32]Compare in this connection the peculiar resistance to change of size shown by the human face in investigation of the after-effects of seen movement, as with the Mach spiral. Indifferent objects looked at after long fixation of the rotating spiral may change either in size or in distance; the face of a person readily changes in distance, but strongly resists a change in size. This suggests the operation of a figural after-effect. It would be significant to investigate here the comparative rôles of figural and groundal fields.

theory of results derived more or less independently in work on grouping, reversible perspective, attentional or conscious differentiation of the visual field, and the nature and behavior of the object. More than this, as we have seen, the figure-ground concept may be basic to a theory of perception, and therefore to a general psychological system. The work of Rubin has been absorbed into the system of the *Gestalt* school, and much has been made of the distinction of figure and ground. The figure-ground phenomenon is regarded by Koffka[33] as the simplest type of mental *Gestalt*, fundamental to human consciousness.[34] Perception in the theory consists in the emergence of a *Gestalt* from a general level (ground).[35] One may turn, therefore, in view of the pivotal position that the figure-ground phenomenon has assumed in psychological theory, to an inquiry into its precise nature and properties.

Experimental Analysis

Preliminary investigation showed that the nature of the figure-ground experience varies according to the time of exposure of a stimulus. This led to an analysis of the figure-ground phenomenon by variation of the time of presentation and, as early became necessary, control of the criterion of report.

Rubin had used exposures of about 4 sec., from the results of which, along with observations of his own, he derived the exposition of the properties of the figure-ground phenomenon, as summarized above. It appeared obvious that shorter times would suffice for the experience of figure and ground.

Materials. In Rubin's experiments, an irregular hole occupying an area of about 1.5 x 1.5 cm. was cut in a card, and the card was placed between a green light-source and a screen, so that the observer saw reflected from (or transmitted through) the screen a meaningless green field within a large black field. The stimuli used here were adapted from Rubin. On white cards were pasted irregularly-formed fields cut from a 1.5 x 1.5 cm. square of black paper; these 'nonsense-figures' were of the order of complexity of those illustrated by Rubin.[36] As stimuli 1060 different forms were used.[37] When in the tachistoscope the black field appeared centrally in a white area 5 cm. wide and 7 cm. high, all framed by the whitish object-holders of the instrument; beyond the object-holders the field was dark.

Apparatus. The exposure-apparatus consisted of a modification of the Dodge tachistoscope. One channel controlled the presentation, another the fixation field. The apparent position of every object was 32 cm. directly in front of the observer. The fixation-field consisted of black cross-lines on a white card. Before exposure the fixation-field was always illuminated; it disappeared 125σ before exposure, and reappeared $(96\text{-}D_t)\,\sigma$ after the exposure (D_t = exposure-time).

The control of exposure-time was by means of a heavy screen which fell under the force of gravity as freely as the slight friction of its guides would permit. Its action was calibrated by means of a chronoscope; its variation was shown to be negligible. A slit in the screen, adjustable in width by means of a vernier, fell from a constant height past a 1 cm. window so that during the period of apposition light from the light-box was admitted to the appropriate channel, illuminating the stimulus. When the width of the slit was less than that of the window, as it was for values of D_t less than

[33]Koffka, Perception, an introduction to the *Gestalt-Theorie*, *Psychol. Bull.*, 19, 1922, 553-570; *The Growth of the Mind*, Eng. trans., 1924, 131 ff.
[34]W. Köhler, Gestaltprobleme und Anfänge einer Gestalttheorie, *Jahresbericht über d. ges. Physiol.*, 1922, 523 ff; *Die physischen Gestalten*, 1920, 183 ff.
[35]Koffka, *locc. citt.*
[36]Rubin, *op. cit.*, Ab. 1. [37]Stimuli were used over in succeeding series.

12σ, the illumination was proportionately reduced. The sources of illumination were 75-watt, 110-volt, frosted incandescent lamps, in separate compartments of a light-proof housing.

All the experimentation was conducted in a double dark-room. One compartment contained the apparatus. The hood of the instrument was in the second compartment, where the *O* sat. An overhead light was kept on in *O*'s compartment; this was not visible during observation of a stimulus, but served between presentations to keep the subject in a mild state of photopia. Since the general illumination of the room was approximately that which obtained during presentation, adaptive variations were obviated.

Observers. The observers were Mr. K. E. Zener (*Z*), Assistant in Psychology; Mr. J. G. Beebe-Center (*B*), Tutor in Psychology; and Dr. F. A. Pattie (*P*), National Research Fellow in Psychology. All were experienced observers.

Since the procedure proved unusually productive from the standpoint of descriptive material, the quantitative study will be accompanied by a consideration of certain of the figure-ground phenomena on the basis of the protocols. Among the quantitative results are included (1) Simplest Figure-Ground experience, (2) Minimal Contour, (3) Continuous Contour, (4) Minimal Localization, (5) Definite Localization, (6) Halo-experience, (7) Good Figure-Ground experience, and finally (8) 'Perfect' Figure-Ground experience.[38]

For each of the first seven topics the general procedure was the same. After preliminary exploration,[39] which consisted in taking descriptive reports of experiences obtained through various times of exposure, the *O* was given a particular criterion of report, and by the method of constant stimuli a set of percentages was obtained for a chosen range of exposure-times. These percentages represented the frequencies with which the designated experience occurred under the given conditions, and from these data a temporal RL was computed by Urban's method.

In each of the tables (I-VII) the first column (D_s) gives the time-values in slit-widths as they were originally recorded: this value is translated into sigma (0.001 sec.) in column 2 (D_t). In the third, fourth, and fifth columns are given the percentages of positive reports for each *O* for every exposure-time. At the foot of the table are, first, the computed limen (RL_s) in slit-widths with the precision-value (h_s) in the same units, and below that the same limen translated into sigma (RL_t).

Series 1. Simplest Figure-Ground Experience. It was very early evident that a reduction of the time of presentation occasioned the dropping-out of some of the features of the figure-ground phenomenon known to be present in long exposures. The question arose as to how far this limitation might proceed without elimination of the essential features of the figure-ground experience. In order to answer this question the *O*s were given rather free instructions:

Instructions. "Your criterion is the simplest experience that you would designate as 'figure-ground experience.' Following the presentation report 'Yes' or 'No' accordingly as the experience was present or absent."

The procedure, as has been explained, was in the manner of a determination of a stimulus threshold, with frequent demands

[38] The actual order of experimentation was not the same for any two *O*s. It was for Z, Series 3, 2, 1, 6, 7, 4, 5, 9, 8; for *B*, Series 3, 2, 1, 4, 5, 6, 7, 9, 8; for *P*, Series 3, 2, 1, 4, 7, 5, 9, 8, 6.

[39] The exploration- and practice-series occupied usually the first 15-20 min. of the period; during this time descriptive reports were liberally taken.

for descriptive reports. In their descriptions the *O*s show striking agreement as to the nature of the simplest figure-ground experience.

(1) The first essential characteristic of the simplest figure-ground experience appears to be a certain degree of heterogeneity in the visual field: the field is so differentiated that two separate, intrinsically homogeneous regions are perceived.

B: "I call the figure-ground experience one in which a part of the field is more homogeneous than the entire field. The relative homogeneity of a part is greater within itself than the homogeneity of the entire field taken in general."

Z: "The essential characteristic of this criterion is the existence of something within the field of vision which can be pointed to as some kind of unit differing from the rest of the field. A mere difference in tint is not sufficient. There must be the appearance of some sort of shape within the figure."

P: "When it is homogeneous I say that there is no figure-ground there. A sort of smudge I don't call figure-ground."

(2) With this character of heterogeneity is always correlated a difference in brightness between the two fields. This was reported by all *O*s.

(3) Shape, in a technical sense as a roughly restricted, designable expanse, was expressly reported by two of the *O*s (*Z* and *P*), and the third *O* found the figure always to have 'spatial position' of a characteristic sort. *Z* required more or less 'form' for the shape. *P* appeared to demand somewhat more 'form;' his shapes apparently were all slightly 'formed.'[40]

(4) All *O*s required a bounding mechanism of a very primitive sort. *P* called this 'contour,' but the descriptions showed his requirement to be very limited: this contour was no sharp gradation, as a rule, and might appear only in a limited portion of the experience. *B* and *Z* reserved the term 'contour' for a sharp gradation;[41] they did not here require contour as they defined it. The evidence indicated that *P* demanded for simplest figure-ground experience a more nearly abrupt transition than did the other *O*s.

The nature of a typical experience may be shown by one of the more complete descriptions:

Z: "Figure vaguely in the shape of a rectangle, though quite irregular. Unequal tint. The greatest blackness was toward the bottom of the figure. Contour extends to a certain extent along the bottom. The top had shape without contour."

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[40]There is here some difficulty in terminology. It appeared that there is needed one term to designate the primitive spatial distribution of a (figural) field, and another to apply to the precise object-like character of a field. Following the tendencies of the *O*s, in the first capacity will be used the term 'shape,' in the second, 'form.' 'Form' corresponds to the German '*Gestalt*.' 'Shape' corresponds more to what others have termed 'expanse' or 'extent,' but is a somewhat more definite term than these. Z remarked: "A *shape* is a patchy affair, a place in the visual field different (here, in intensity) from the rest of the field, with a minimum of spatial characters. To me 'form' implies a rather definite contour."

[41]In the following the term 'contour,' unmodified, will be used as designating the sharp, definite affair. See below, p. 205 f.

Figure-ground was reported absent when the essential features as mentioned above were not present:

Z: "A simpler sort of experience is a sort of black that fades out;" "a borderline case; suggestion of a certain focus of greatest black; just diffused out without constituting any figure at all. The field was not uniform, but there was no definite shape to the locus."

B: "Field was darker and was not homogeneous, but there was no part of the field that was more homogeneous than the rest."

P: "No definite contour; sort of a smudge;" "homogeneous all over."

TABLE I*
Simplest Figure-Ground

D_s	D_t	Percentages		
		P	*B*	*Z*
0.4	8.7		.04	
0.5	9.3	.00	.24	.04
0.6	9.9	.08	.56	.24
0.7	10.5	.56	.96	.60
0.8	11.1	.85		.88
	RL_s	0.698	0.561	0.675
	h_s	8.437	17.062	6.827
	RL_t	10.52	9.68	10.37

TABLE II
Minimal Contour

D_s	D_t	Percentages		
		P	*B*	*Z*
0.6	9.9	.04	.08	.00
0.7	10.5	.12	.12	.04
0.8	11.1	.68	.36	.16
0.9	11.7	.88	.80	.64
1.0	12.3	.88	.96	.96
	RL_s	0.771	0.815	0.869
	h_s	4.910	5.883	8.508
	RL_t	10.95	11.22	11.55

TABLE III
Continuous Contour

D_s	D_t	Percentages		
		P	*B*	*Z*
1.0	12.3	.24		
1.1	12.9	.24	.08	
1.2	13.5	.68	.52	.08
1.3	14.1	.76	.68	.48
1.4	14.7	.88	.92	.52
1.5	15.3			.76
1.6	15.9			.80
	RL_s	1.157	1.229	1.381
	h_s	3.118	5.937	3.350
	RL_t	13.29	13.71	14.61

TABLE IV
Minimal Localization

D_s	D_t	Percentages		
		P	*B*	*Z*
0.5	9.3		.08	
0.6	9.9	.18	.30	
0.7	10.5	.54	.64	
0.8	11.1	.72	.88	
0.9	11.7	.94		
1.2	13.5			.20
1.4	14.7			.36
1.6	15.9			.54
1.8	17.1			.50
2.0	18.3			.68
	RL_s	0.707	0.653	1.683
	h_s	5.457	6.100	1.030
	RL_t	10.57	10.24	16.41

The quantitative results are brought together in Table I. The values of the limens reflect the agreement of the *Os* in regard to the nature of the figure-ground experience at its most elementary level. The conclusion from these results is that under the given conditions of illumination, distance, and stimulus, a figure-ground experience of the most primitive nature arises from an exposure of the order of 10 σ. The h-values indicate a relatively rapid rate of change of the phenomenon in this region. The descriptive features of the typical experience

*Each RL is calculated from 25 series. The same is true of Tables II and III. The remaining tables (IV-VII), where the values of h are not so large, involve 50 series.

are two heterogeneous fields, differing in brightness, one of which fields (the figure) has 'shape,' and a 'contour,' or a bounding mechanism, of a primitive sort.

Series 2. Minimal Contour. Since, as the first series showed, a definite contour is not essential to the simplest figure-ground experience, the question naturally follows as to what order of exposure-time will give such contour in at least a portion of the experience. The *O*s were instructed:

Instructions. "Your criterion is minimal contour: any contour at all is to elicit the judgment 'Yes,' entire absence of contour the judgment 'No.' "

Minimal contour consists in a sharp gradation, a definitely marked boundary, between figural and groundal fields; the contour is not necessarily continuous. This is shown in the reports of the *O*s:

B: "An experience in the upper part of the field of a sharp gradation that could be called a contour."

Z: "The only contour existed on the left-hand side. The boundary here was quite definite. The rest of the figure blurred out, in a sort of smudgy way, in the field."

P: "A sort of irregular square on a background that was not much lighter than the square itself. I think the place that I caught the boundary was mainly on the upper side. It was not as definite a contour as I had experienced where there was more difference between figure and ground."

The figure-ground experience at this stage has other characteristic features. It consists typically, under these conditions of exposure, of a dark grey or very dark grey field of rather indefinite shape, upon a grey or less dark grey background; the intensity is relatively low.

The results of the psychophysical procedure are shown in Table II. There is close agreement among the *O*s, showing that this criterion of minimal contour is a definite one. For each *O* the limen for figure-ground experience with minimal contour is greater than the limen for simplest figure-ground experience, showing, in accordance with the descriptive data of Series 1, that in the statistical average the simplest figure-ground experience occurs before definite contour has time to develop.

Series 3. Continuous Contour. In the longer exposures of the last series the contour was frequently described as completely enclosing the figure; in this series the criterion of report in the determination of a limen was that of figure-ground experience with such continuous contour. The *O*s were instructed to report 'Yes' or 'No' accordingly as they experienced figure-ground with complete, continuous contour. The contour here, except for the added requirement of extending all around the figure, is of the same character as that of the last series. The experience as a whole, however, is somewhat altered. In general, in the

figure-ground experience with complete contour, the perception is that of a dark grey or black field, of definite shape and greater or less homogeneity, which rests upon a light grey background, and is separated from that background by a contour that is definite and continuous.

As is shown in Table III, the RL's for all *O*s are very close together, and in every case are somewhat longer than those for minimal contour. The variations between *O*s are in the same order in the two series, a fact which correlates well with the general agreement of the *O*s in their descriptions of the experiences. The figure-ground experience with complete, continuous contour has been found to arise, under the conditions, with an exposure of 13-14 σ.

One may now turn to a descriptive analysis of the contour experience. Schumann, in his work on grouping,[42] found that sharper boundaries assist in differentiating a group from its background. This peculiar character of the bounding lines he attributed to better accommodation. Zigler[43] confirmed Schumann in finding group-formations to be characterized by alterations in the boundary lines, and found further that the contour-lines are blacker, clearer, more extended or broader, and more sharply defined at the edges. Such was the extent of knowledge of contours before Rubin published his study.

Rubin[44] found: (1) The figure seems to be due to the operation of the contour. The contour may be regarded as giving form to the figure-surface. (2) This contour may sometimes have so little to do with the ground that the ground extends uninterruptedly behind. (3) The contour has no breadth. (4) The contour has no color. (5) The contour differs from the surface in that the latter is a unity, but the contour may have several components.

Rubin seems to have made a distinction between contour (*Kontur*) and edge (*Rand*). In scotopia (and sometimes also in photopia) the observer may experience blurred surfaces (*Flächenfiguren*), in which case Rubin speaks of an edge, which, however, preserves close relationships to definite contour.[45] While such a distinction may be of practical convenience, one is forced to point out that no line of demarcation exists between the sharp gradation that Rubin calls "contour" and the more gradual transition that he calls "edge." However, in this study the practical distinction is more or less maintained, for the *O*s in speaking of the less definite affair usually employed such expressions as 'hazy contour,' 'fuzzy contour,' etc.

(1) The term 'contour,' unmodified, is designated as "abrupt gradation" (*P*), "abruptness of transition" (*B*), "definite outline," "continuous break between black and white

[42] *Zsch. f. Psychol.*, 23, 1900, 23.
[43] See above, p. 197.
[44] Rubin, *op. cit.*, 105-7.
[45] *Idem.*

with sharp distinction" (*Z*). This agrees with Rubin's use of the term as designating an experience which is without breadth and without color.

(2) A contour may have breadth; that is to say, the gradation from black figure to white ground is gradual, extending over an interval.

Z: "The figure was uniform grey, not of surface-character, but like dark woolen cloth, a little bit fuzzy. The edge seemed to have a definite existence, to be a little bit in addition to being the edge of the figure."

B: "The brilliance of the blob was greater than that of the rest. The blob stood out, was three-dimensional, though indefinite. The degree of heterogeneity between blob and background was very little. The gradient was gentle rather than steep. The outline was fuzzy, not homogeneous in brilliance; that is, the brightness between background and blob was not homogeneous either."

(3) The contour may have tridimensional properties.

Z: "A line around the edge of the figure, a thing which was a little bit darker than the rest of the figure, a little bit more solid, firm, with more substantiality. It definitely belongs to the figure, and is not part of the ground in any sense."

B: "Dark blob, continuously contoured. Blob seemed to have object-character, three dimensions. The blob was homogeneous. The contour was very much as if it was not an abrupt stop of an object, but like that of an object that was three-dimensional. Hard to say anything, except that it was a *three-dimensional contour.*"

(4) The separation-mechanism may be a very gradual shading of one field into the other. Here the grey quality of the contour is more marked.

P: "Hazy contour. Noticeable brightness-difference between figure and ground. The contour was not sharp: there was not a distinct contrast between white and black. The contour was not definite, like a line, but represented more or less by an area. Grey, intermediate between the dark grey of the figure and the light grey of the outside."

B: "To the left of the vertical line was a dark patch which merged abruptly into the background on the left. Down towards the right lower corner it merged gradually to a darkish grey, a fog."

This affair may or may not be called a contour.

Z: "The figure was a dark grey of rather misty appearance. It seemed to merge to a certain extent into the field, especially on one side, where it sort of streamed off, like a whiff of cloud. The field itself was unevenly dark grey, nearly as dark as the figure on the edges of the field. It was somewhat lighter immediately adjacent to the figure. I object to calling that a contour—it is a fuzzy edge."

(5) The merging may be so gradual that no 'step' is perceived. (If this condition prevails over a relatively large portion of the experience, differentiation of figure and ground is usually absent.)

Z: "A small part of the figure had a contour which was definite. In one spot near the top the thing was very vague and misty—no contour; the figure sort of flowed off into the ground." "It very definitely faded out. A rather square-looking figure. Dark in the middle, grading out to the

edge with decreasing character. The whole thing was not solid, or surfaced, or smooth, especially near the edges. . . . I should hesitate to call that a figure; it was more like a 'shape.'"

B: "A little white spot, surrounded by a dark ring, ring merging into a white. At some points there was a definite step, a change in brilliance, at others there wasn't any step. I couldn't say whether the step is white-to-black or black-to-white."

P: "No contour. Card with field, brightest around the edges. Toward the center it got gradually darker. I wouldn't say it's a figure-ground experience. Just a black nucleus which very gradually shaded out. No contour."

This continuum of experiences, from definite contour to a gradual merging of the fields, is roughly correlated with decreasing time of exposure.

Besides differentiation by the grade of transition, there is that by the extent of occurrence of contour in the experience; all stages are shown between part-contour and complete contour. With relatively long times the contour is continuous; this ranges from the most definite sort, in which tint-contrast is uniform and continuously abrupt, through intermediate stages to the 'hazy' or 'fuzzy' type, in which the tint-contrast is low and less uniform.

Incompleteness of contour may appear in several forms. In the course of a sharp or of a 'fuzzy' contour there may occur complete gaps—one, two, or more. In the extreme case there is but a 'corner' of contour in the experience. Frequently both sharp and 'fuzzy' contour are present in an experience, though the contour is not complete. With shorter times the 'fuzzy' or 'foggy' type of contour is present in but a limited portion of the field. In those portions of the field reported as without contour the one field shades off into the other so gradually that there appears no 'step' of such height as to become contour; it is in this region that one finds the lower threshold of figure-ground experience. Just below this threshold the gradation may be sufficient to give a certain degree of heterogeneity in the field of vision, yet the experience is not one of figure and ground. Typically the experience here is one of a dark nucleus standing in the center of the field and grading imperceptibly into the periphery. As one *O* described it, it is "like a smudge of ink—it reaches the maximum in the center, and fades out so that there isn't any place where there is a contour" (*P*). This experience merges into one of utter homogeneity.

That contour is a function of the brightness-difference between the two fields is suggested in the reports of all the *O*s. The lower forms of contour are correlated with a low degree of brightness-difference. In these experiences the entire field is usually of low intensity, and of grey, foggy character. At the lowest stage of the phenomenon one field passes imperceptibly into the other, and no 'step' is experienced between the dark

grey of the one area and the lighter grey of the other. When the difference in brightness has reached a certain value, however, a 'step' appears, and the figure is marked off from the ground by a line of separation. Later, when the brightness-difference is greater, the separation is more marked, and the figure takes on unit-character, or thing-character, in higher degree.

Series 4. Minimal Localization. The fact is significant, in view of the work of Rubin on the figure-ground phenomenon, that in the three series just described but casual mention has been made of depth-localization of figural or groundal fields. Contrary to what one might be led to expect from previous work on this subject, a difference of depth-localization of the two fields, though it may at times occur, is not necessary in the primitive stages of figure-ground experience. The appearance of such a difference in localization was investigated in this series. The instructions were:

Instructions. "Your criterion is a difference in localization, as regards depth, of the field which is figure and the field which is ground. When such a differentiation occurs you are to report 'Yes;' in the absence of such differentiation, report 'No.' "

Minimal localization consists typically of a 'standing forth' of the figure from its ground; numerous other conditions, however, satisfy the criterion of a difference in localization, as the reports of the subjects showed:

B: " 'No'-judgments mean either (1) that the figure and ground are seen as one plane or (2) that the figure has no place. 'Yes'-judgments mean that the figure and ground are separate in one of two senses: (1) one back or in front (usually the ground is behind, but not always) or (2) the ground has no localization."

P: For this *O* the figure always, or nearly always, stood out from the ground; "a rather irregular figure, with a good many nicks on the right side. The right side had a good contour, and here it seemed to stand out from the ground." He reported localization as absent when the experience consisted of "a sort of blur, shape of a round object; no definite contour; didn't stand out from the field."

Z: The difference of localization might consist "in not being on the same plane, or . . . that the figure is on a definite surface and the ground on a definite surface but not on the same surface. The figure may be on a plane and the ground may not have surface-quality, is . . . indistinct."

The 'standing out' of the figure was by *P* correlated with brightness-difference, by *Z* with *Eindringlichkeit*. The background of the experience in most cases shows peculiar properties of texture. *P* often referred to the background as 'blurred,' *B* and Z called it 'filmy.' *B* emphasized the texture-difference of figure and ground in these experiences:

"Dark patch on a lightish grey background . . ., looked like lighted space, or opaque glass." "With one attitude or another I can get surface or film."

The results (see Table IV) show some differences among the *O*s. For *B* and *P* the limen for minimal localization is slightly

greater than 10 σ; for the third O (Z) it is over 16. This difference may be due in a measure to the fact that Z's experiences were somewhat more definite affairs than those of the other Os: for the most part both figure and ground were surfaced, and on different planes. For him, experiences in which the figure was localized in a plane but the ground was not at all localized were infrequent. Since surfaced character of the fields, and particularly of the groundal field, is correlated with longer times of exposure, the larger limen of this O is partially accounted for.

The principal conclusion from these data is that a localization-difference of figural and groundal fields arises at a comparatively early stage in the development of the figure-ground phenomenon. The fundamental and striking feature of this difference of localization is the protrusion of the figure in the field of vision. The ground, on the other hand, is recessive; its negativity may be such that it is not at all localized, and in this event it manifests usually an indeterminate, filmy character of texture.

Series 5. Definite Localization. It was suspected that something perhaps of fundamental nature might be found in the difference between that localization-phenomenon in which the figure protrudes from an indeterminate background, and that experience in which the two fields are definitely perceived as standing upon different planes in space. The former is what has just been subsumed under minimal localization; the latter is the more familiar phenomenon of figure-ground localization, as it is described by Rubin, and as it familiarly occurs with objects of our visual world. To test this suspicion, a series was given with the following instructions:

Instructions. "Your criterion is localization as regards depth, interpreted in distinct terms. To elicit the judgment 'Yes' the experience must be of a surfaced figure definitely situated *in front of* its ground. The ground may be either surface or film, but it must be definitely *behind* the figure. 'No'-judgments are to be made when figure and ground are on the same spatial level (as regards depth) or when figure-ground differentiation fails. In case the figure appears *behind* the ground, this is to be especially reported as such."

Definiteness of localization is accompanied by marked changes in the nature of the whole experience. The absolute brightness is greater; the brightness-difference is more marked. The contour is sharper, more distinct, and continuous. When a texture-difference is a part of the experience, it is the figure-surface which has plane-quality, while the ground usually is filmy, insubstantial. Yet here the localization of the ground is not indeterminate; the ground tends to be tridimensional, and may be termed 'hazy,' 'foggy,' 'filmy,' or 'translucent,' but this does not mean that it is unlocalized. In fact, it may be well-

localized in a general region of space; there is a difference between a rough localization and no localization at all.

The instructions have ruled out those cases in which localization of the figure without localization of the ground, and localization of the figure back of the ground, are taken as positive reports. This alone would tend to make the limens larger. The requirement of definite character to the localization is the prime feature, however.

The results are to be found in Table V. The disparity among the *O*s is apparent. *P*'s limen is but slightly larger than that for minimal localization. The reports showed his criterion here to differ but slightly from the previous one:

"Cases of best localization have big differences of brightness between figure and ground. The contour is sharp and distinct all the way around. The figure is homogeneous. The field is continuous behind. The thing seems to be unsteady with respect to motion. In other cases I don't get the idea that the field is continuous. When the contour becomes just a little bit more vague it seems merged."

B's limen was greater; his criterion differed in the point that here the ground, though film, was in a sense localized in space.

"The dark blob was surrounded by a light grey film which didn't have any surface-qualities. The blob seemed to be at the front edge, or in front, of the film. The film had no particular place except that it extended behind the figure."

The localization-experience varied a good deal for *Z*, but the typical case appeared to be one in which both figure and ground had surface-character, with the figure localized slightly in advance of the ground:

"Figure slightly in front of the ground. Homogeneous, contoured figure. Ground somewhat surfaced."

The data are by no means conclusive on the question whether this definite sort of localization is fundamentally different from that considered in the previous series. The simplest supposition would be that minimal localization is a primitive and definite localization a more advanced stage in the development of localization. There is no evidence which would controvert such a view. On the other hand, the nature of minimal localization as a protrusion of the figural field, and the suggestion of intimate connection with insistence and *Eindringlichkeit*, point toward a possible relation to attention, while subjective localization, along with more common phenomena of localization, has not ordinarily been regarded as having anything to do with attention. The difficulty here can be removed only by further investigation.

A descriptive analysis on the basis of the protocols will serve to emphasize the variety of localization-experiences and the complexity of the whole problem of localization. Rubin in

his discussion of subjective depth-localization of figure and ground mentioned four types of experience: (1) figure and ground are on the same plane, (2) the figure is in front of the ground, (3) occasionally, the figure is behind the ground, and (4) the ground is in general less definitely localized than the figure. A number of additional types of depth-localization present themselves.

As has been suggested above, a difference in localization of the two fields may occur in two principal forms. (a) In the plainest instance both figure and ground are perceived as definitely situated in space, and on different levels of depth. Here the figure is nearly always in front of the ground; sometimes the ground extends uninterruptedly behind the figure, more often it does not do so, but stops at the contour. Occasionally the figure is back of the ground. (b) Mention has already been made of the case in which the figure is perceived as definitely situated in space, but the ground is not localized.

When there is no difference in depth-localization, the experience may be (1) that of a figure and ground definitely situated in space, on the same plane, or (2) one in which there is no definite localization of either figure or ground.

Certain other phenomena are striking enough to warrant mention here. There is frequently a difference in depth-localization between separate portions of one of the two fields. Though usually the figure is on a plane perpendicular to the optical axis, this is not invariably the case. At times the figure is on a tilted plane.

P: "A submerged figure . . ., as though the figure had been placed in liquid, with one corner sticking out."

Rather frequently the figure is convex, especially near the boundary:

Z: "The figure seemed like a solid object rounding off at the edges into the field." "The figure was like some sort of 'pillow'—the edges were on the same plane as the ground, but the center bulged out."

Occasionally the figure is concave:

B: "Curved so that the two edges were nearer to me than the center."

Particularly with longer times of exposure the figure is definitely tridimensional:

B: "Head of a monkey; good—has thick thickness, especially over the head of the monkey."

P: "Like a glob of metal with a handle."

Often the ground is not a plane surface. It may be curved, usually so as to be concave towards the *O*.

Z: "The ground seemed to fall away from the figure slightly around the contour."

Sometimes the ground has volume, and extends all around the figure:

B: "The figure was situated in a grey mist . . both behind and in front, mostly behind;" "in a sort of atmosphere."

Z: "Figure not homogeneous, seemed rectangular in shape; no definite contour. Appearance of a black object seen through a mist. The ground was behind and in front of the whole thing. The whole thing was blurred and smudged; no accurate localization."

Depth-localization of figure and ground is an unusually labile affair. With any given conditions of presentation is mani-

TABLE V
Definite Localization

D_s	D_t	Percentages P	B	Z
0.8	11.1	.22		
0.9	11.7	.50		
1.0	12.3	.84		
1.1	12.9	.90		
1.2	13.5	.98		
2.0	18.3		.16	
2.5	21.2		.26	
3.0	24.1		.66	
3.5	26.9		.82	
8.0	51.4			.16
9.0	56.5			.32
10.0	61.6			.42
11.0	66.6			.52
12.0	71.5			.60
	RL_s	0.898	2.807	10.901
	h_s	5.062	0.973	0.208
	RL_t	11.72	22.96	66.06

TABLE VI
Halo-experience

D_s	D_t	Percentages P	B	Z
0.5	9.3			.04
0.6	9.9	.16		.24
0.7	10.5	.36	.12	.48
0.8	11.1	.34	.32	.52
0.9	11.7	.52	.64	
1.0	12.3	.60	.92	
1.1	12.9	.80		
	RL_s	0.883	0.849	0.783
	h_s	2.263	5.990	3.658
	RL_t	11.62	11.42	11.02

TABLE VII
Good Figure-Ground

D_s	D_t	Percentages P	B	Z
0.7	10.5	.08		
0.8	11.1	.20		
0.9	11.7	.54		
1.0	12.3	.68		
1.1	12.9	.96		
1.4	14.7		.38	.20
1.6	15.9		.70	.34
1.8	17.1		.72	.54
2.0	18.3		.84	.64
2.2	19.5			.74
	RL_s	0.885	1.478	1.813
	h_s	5.106	1.426	1.317
	RL_t	11.64	15.19	17.18

fested a great variety of experiences. Only when taken statistically is the depth-phenomenon shown to be correlated with time of exposure. One cannot establish compulsory conditions for the demonstration of a given type of depth, though one may maintain conditions that favor some desired experience.

The relationship between depth and texture has been touched upon, but the matter needs extensive investigation. The

evidence shown here is in support of the generalization that indeterminate localization is correlated with filmy texture, and the more definite localization with greater surface-quality.[46]

Depth has also an important connection with object-character. The thing-character of the figure is very probably related to that field's more precise localization. Later it will be shown that changes of object-character are accompanied by changes in the spatial properties, of the figure.[47]

Series 6. Halo-experience. *P* had reported spontaneously, in previous series, what he called a 'corona' or 'halo:'

"A black spot, then a white halo around it, and then a dark field or ground." "A sort of corona;" "corona seems attached to the black object in the center. Corona and black object seemed to stand out from the black background. Corona is thrown out from the black object."

B reported a sort of 'contrast,' which he later called an 'aureola' or 'halo:'

"Field is much lighter around the figure—contrast." "A slight aureola;" "a dark, very dark grey patch, roughly square in shape, surrounded by a light grey, quasi-circular field, it itself surrounded by a darker grey field. The transition between black and halo was sharp. The transition between halo and dark grey was gradual."

When Z was asked he immediately reported that he had experienced "something in the center, lighter about that, and darker on the outside."

Instructions. The *O*s were instructed: "Report 'Yes' when you perceive an aureola or corona-effect in the field; 'No' when you do not."

There are perhaps two extreme cases of this experience. (1) What may be called the corona-experience proper begins as a lightening around the black figure, and extends outward in all directions, decreasing in brightness toward the periphery, and shading off into the marginal darker ground. In this form the light grey effect occupies a rather large proportion of the area outside the black figure.

Z: "Outside of the figure the field was light, the lightness fading out not so very gradually—like a logarithmic curve—toward the edges."

P: "Dim corona . . ., this white streak sort of shaded off into the background. Figure and ground had just about the same blackness."

B: "A lighter patch surrounding the dark figure, and between the dark figure and a medium grey background. Extended a good half-centimeter from the figure, and from the figure-side was sharply differentiated, but merged distinctly more gradually into the background."

(2) The halo or aureola may consist of a narrow bright streak just outside the black figure.

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[46]See the experiments of Hering, Hermann's *Handbuch der Physiologie*, V, i, 574 f.; Katz, *Zsch. f. Psychol., Erg. Bd.*, 7, 1911; Rubin, *op. cit.*, 51 ff. and note, 57; M. F. Martin; Film, surface, and bulky colors and their intermediates, this JOURNAL, 33, 1922, 451-480. Rubin did not find the above-suggested correlation to be absolute.

[47]See below, p. 220f.

Z: "A ring or a lighter band around the darkest inner part of the field."
P: "Small streak of white, more or less all the way around."
B: "A slight halo of light grey;" "very small aureola, very concentrated around the figure; practically a contrast-line."

The first form of halo-experience obtains when the time of presentation is brief, and the ground is of low intensity. The second form appears with longer times. Between these two classes all intermediates appear. The instructions in the experimental series included all varieties of the experience. A complete halo was not required: the typical experience was a slight halo in one portion of the field, adjacent to a relatively pronounced contour-line.

The results of the psychophysical procedure are shown in Table VI. The limens for the three *O*s are in fair agreement. *P* showed a good deal of variability, due to uncontrolled changes of attitude; this was sufficient to produce an inversion in the percentages. The other *O*s showed fair regularity. The halo-experience in its liminal form is shown as occurring with a presentation of about 11 σ.

The factors determining halo-experience are in part obscure. Attitude plays a part. The degree of light-adaptation is an important feature. The halo-experience is probably a form of the phenomenon of simultaneous contrast, and the results show liminal conditions for the appearance of the phenomenon. The course of the phenomenon with longer times of exposure is yet undetermined.

Series 7. Good Figure-Ground Experience. Under "Simplest Figure-Ground experience" the most primitive stage of figure and ground has been discussed. It is important to consider more advanced stages of the phenomenon. 'Good figure-ground experience' was rather arbitrarily defined for the purposes of this experiment as an experience simultaneously containing all the features of simplest figure-ground experience in highly developed state.

Instructions. The *O*s were instructed to report the concurrence of marked difference in brightness, distinct definition of figure and ground, well-marked contour all the way around the figure, and localization of the figure in front of the ground. The last-named factor of localization was found not to be essential, and was not insisted upon.

It must be noted that the purpose of the instruction was not to define, but primarily to 'grasp' the phenomenon, and the results are in accordance with this presumption. What here is regarded as 'good figure-ground' is not merely the concurrence of a few factors that seem logically necessary, but a unitary affair in which can be demonstrated various descriptive features.

(1) The primary condition of 'good figure-ground experience' seems to be a relatively large difference in the brightness of the two fields. This was emphasized by all *O*s. *Z* reported at one time:

"My criterion is reduced primarily to judging on the basis of a certain absolute difference in tint between figure and ground. The figures almost all possess continuous contour and difference between figure and ground. . . . Difference in tint is a function largely of the uniform blackness of the figure and the whitishness of the ground, probably predominantly of the latter. There seems to be more difference, more variation, in the tint of the ground than in the tint of the figure. There seems to be a difference between neutral greyishness and a white."

(2) Another essential condition is continuity and well-marked character of contour. For *P* this was not emphasized as much as brightness-difference. The contour required was not as sharp a contour as for the others.

(3) Heterogeneity, or distinctness of the two fields, follows directly upon the brightness-difference between the surfaces.

(4) The intrinsic homogeneity of figural and groundal fields was emphasized. *B* remarked:

"I should ca'l a 'good figure-ground experience' one in which a certain portion of the field was homogeneous and abruptly heterogeneous. I would say that the 'goodness' of a figure-ground experience is a function of the homogeneity of the figure and the heterogeneity between it and the ground."

(5) A rather general factor is definite ('formed') shape; the figure has a distinctly perceived shape that can be named:

B: "Shaped like a pear laid on its side."

(6) The figure protrudes, stands out from the background with a certain insistent character. This is the property already referred to under the section on minimal localization. Perhaps to be distinguished from this is (7) a definite difference of depth-localization of figure and ground, which in 'good figure-ground experience' may be present, but is not essential.

Z: "No definite difference in localization, but didn't seem exactly in the same plane."

B: "This space thing doesn't come in."

P did not mention localization aside from the 'standing out' property discussed above.

The limens (see Table VII) show fair consistency. *P* shows a difference, in that his limen is smaller than that for continuous contour; this is due to the fact that here the continuous contour required was not of the same definite character as before—the contour was continuous, but might be of a 'fuzzy,' 'hazy' kind, not a sharp gradation. All the limens are below the values for 'definite localization,' reflecting the fact that definite localization—contrary to the anticipation of the original instructions—is not a necessary property of 'good figure-ground.'

The properties of 'good figure-ground experience' are marked brightness-difference of the fields, definite, or fairly definite, continuous contour, heterogeneity of figural and groundal surfaces intrinsic homogeneity of the separate fields, 'formed' shape, protrusion of the figural field, and, at times, separation of the two fields in depth.

'Perfect' Figure-Ground Experience. Out of the study of 'good figure-ground' arose the question whether there could be isolated, by the procedure previously employed, conditions for the production of an experience that could be regarded as *'perfect'* figure-ground experience—the phenomenon at its highest stage. 'Good figure-ground,' as has been demonstrated, may or may not show a definite difference of localization between the two fields; it represents, moreover, a rather arbitrary point in the curve of development of the phenomenon. The problem here was the determination of those conditions of presentation-time through which an experience might be obtained containing in their state of highest development the various features which have been found to enter into the figure-ground phenomenon, among which are primary heterogeneity, brightness-difference, contour, shape, localization, and intrinsic homogeneity of the fields.

Series 8.

Apparatus. The apparatus was the same as before, except that time was controlled with a pendulum instead of the screen. The pendulum tripped metal contacts which were adjustable along the graduated arc of its excursion; the contacts turned on and off lamps so as to illuminate the presentation-fields at the desired times. The lamps used were special high-speed, hydrogen-filled, 150-watt, 110-volt, tungsten lamps. The fixation-field was illuminated except for an interval from just before until just after the presentation-period. The intensities of presentation-field and fixation-field were equated by means of a diaphragm. Thirty-two exposure-times were used, from .091 to .445 sec., by steps of about .01 sec.

Instructions. The *Os* were given the following instructions: "Give absolute judgments of your experiences on the following basis: 'Perfect'—what you would regard as *perfect* figure-ground experience. 'Very good'—not quite perfect. 'Good'—about as much less good than 'very good' as 'very good' is less than 'perfect.'" Report of failure was permitted.

In order to obtain a crude indication of 'perfect' figure-ground, and of the rate of change of the phenomenon with increase of exposure-time, 25 judgments were taken, under the above instructions, for each of the 32 times.

The figure-ground experiences under these conditions of exposure showed no essential differences from the ones previously obtained. The fundamental properties, as found in simplest and in good figure-ground experience, played the same rôles as before. It is true, their character showed some change, for the most part in the direction of greater definition and intensity, but

there appeared no new significant features which would serve to distinguish a figure-ground experience that could be regarded as 'perfect.'

The *O*s agreed that the instructions could not be carried out in an absolute sense in the report of experiences of 'perfect' figure-ground, because any definition of such 'perfect' experience is not absolute, but relative. That is to say, the designation of 'perfect,' in every case, is but an indication of the comparatively high position which a given experience is said to occupy in a particular series.

In this series, with the range of exposure-times as stated, reports of 'perfect' figure-ground experience (in the relative sense) occur with increasing frequency as the times of exposure increase. Reports of 'good' figure-ground experience occur only for the shorter times. Reports of 'very good' occupy the intermediate range. The conclusion is that as time of presentation is lengthened, the 'goodness' of the experience is increased. This increase, however, proceeds at a constantly diminishing rate, being rapid at first, falling off gradually, and approaching zero near the end of the temporal series. The increase of 'goodness' is shown essentially in progressive alterations in the fundamental features of the experience.

The figure-ground experience here called 'perfect' is one of a homogeneous, very black figure, with definite, sharp, and continuous contour, standing forward from a ground which is a homogeneous white. There is a decided brightness-difference between the two fields. In this much there was entire agreement among the *O*s. Here, however, divergence began, a fact which establishes further the arbitrary nature of any designation of 'perfect' character to a figure-ground experience. *B* and *Z* emphasized a characteristic texture. For *Z* the black figure had "surface-character," while the ground was "thick film." *B* described the figure as "alive," "lively," "like shiny paper," the ground as "milky," "pearly," "translucent." *P* emphasized especially the definite character of the contour, basing his judgment of 'perfect' figure-ground chiefly upon this and brightness-contrast. *B* regarded the 'perfect' experience as a compromise of brilliance-difference with depth and texture. Depth and translucency of ground (the essential feature of the texture-difference) vary concomitantly (he reported), and these vary reciprocally with brilliance. *Z* appeared to judge mainly upon depth and texture-difference between figure and ground.

A poorer experience (judged 'very good') is one in which the figure is homogeneous, black, and with continuous contour. The brightness-difference between figure and ground is good or medium. Again there was failure of agreement among the *O*s in regard to the nature of secondary features. *P* reported that the

figure stood out from the ground, *B* that the figure stood out very little from the ground or that the two fields were on the same plane, and *Z* that the fields were on the same plane or that the ground was not distinctly localized either in the same plane as the figure or in another plane. *B* reported the absence of marked difference in texture: both figure and ground were matte, the black having a 'dead' character. *Z* reported the figure as "rather luminous," the ground as milky with duller finish. *P* emphasized the fact that the contour, though continuous, was not quite perfect, having a bit of indefiniteness about it. This was his chief characterization of the experience; *B* relied mainly upon the poor depth and matte texture as criteria of judgment; *Z* depended upon texture-difference and lack of depth.

The next type of experience (judged 'good'—but not to be confused with 'good' figure-ground experience as heretofore discussed) was that of a usually inhomogeneous, smudgy figure of not very intense blackness, and with continuous contour. Brightness-contrast between figure and ground was not great. The ground was not white, but a medium grey. *B* reported sometimes no depth, sometimes best depth, for this experience. *Z* reported the two fields as on the same plane. *P* found the figure to stand out as in all cases. *B* reported that the background was not very distinct from the figure; rather, the two fields formed a unitary whole. *Z* reported further that figure and ground had a dull finish. *P* based his judgment chiefly upon the fuzzy, indefinite character of the contour. *B* reported usually that the contour was not strictly sharp. He distinguished this experience by low intensity of the black and undifferentiated yellowish background. *Z* relied for his judgment essentially upon the lack of difference in depth and in texture.

'Perfect' figure-ground experience is characterized by homogeneity of the fields, intensity of the figural field, definiteness of the (continuous) contour, decided brightness-difference of the fields, protrusion of the figure in front of the ground, and, perhaps, texture-difference, in which the figure is of surface-character, while the ground is filmy or translucent. As these features are reduced in value, the experience moves downward in the 'goodness' scale. A given experience can be judged only by reference to this scale, and the scale itself is a function of the particular range of conditions included in the series. Only by an extension of series can one determine the full course of development of the figure-ground phenomenon.

Series 9. In this series the stimuli were presented for much longer times, varying from a few seconds to more than two

minutes, to test whether such extension of exposure would reveal any fundamental change in the course of the figure-ground phenomenon.

Apparatus. The apparatus consisted of the tachistoscope as before, with a new method of control of exposure. A double-throw switch was operated by *E* to remove the illumination of the fixation-field and immediately to illuminate the exposure-field; simultaneously with this was completed a magnetic circuit which traced an exposure curve on a chronograph. On the same chronographic record a seconds pendulum traced a time line. Two other simultaneous tracings, actuated by finger keys, gave points of orientation in the exposure.

Instructions. The instructions were: "Observe the presented stimulus and during the observation report your experience. The stimulus will be maintained for a time of several seconds. Report any change in the experience during the period of presentation."

This procedure revealed nothing new; there is little to add, aside from confirmation, to the results of the previous series. The fundamental features of the figure-ground phenomenon were present in essentially their previous form, with expected minor differences of intensity and degree.

It is interesting to note that the 'goodness' of the experience does not continue to increase with extension of the exposure-time. This, indeed, was suggested by the 'law of diminishing returns' found in operation in the previous series. With prolonged staring the intensity of the field decreases (due to retinal fatigue), and brightness-difference, contour, and shape are reduced. Depth, on the other hand, may be greatly augmented, so that evidence is added to that previously obtained in support of the statement that the various features in the figure-ground phenomenon, though at times concomitantly varying, are not absolutely bound.

There were obtained certain data which, though incidental to the principal question of this section, are of interest in the general problem. All *O*s spontaneously reported, with these protracted exposures, a reversal of the experience in which white became figure and black the ground. In fact, it is the general rule, in an extended exposure of this sort, for the experience to alternate between black as figure and white as figure. At times this reversal is extremely rapid, too rapid for report, like a flutter; but usually the fluctuation is very irregular, with changes occurring on an average of once in 2 or 3 sec. Frequently only a portion of the white takes on figural properties, while in another region of the stimulus-field a black figure is still present.

The experience usually takes on object-meaning:[48] It looks like, or 'is,' some familiar object, and can so be named.

[48]The expression 'object-meaning' is here used to indicate the reference of an experience to some particular object, by being named as that object, or being reported as like that object. The intention is to avoid the implications which the use of the unmodified term 'meaning' might involve, for this term has frequently been employed by writers who main-

B: "Black figure. Getting meaning—caricature of a face."
P: "Looks like a bud growing off a tree."
Z: "There was a bewilderment at first, and almost immediately it jumped into a Mutt-and-Jeff motif. Then the figure became more definite and clear and more uniform in color."

Object-meaning may change rapidly; report of such change is ordinarily to be correlated with a change of dimensionality.

Z: "This figure first appeared to be a duck. Then some kind of inner closed object. Then the metal handle of some motor. In all these it tends to become an object in three dimensions, and can be shoved back into a flat picture. Here it turned into a silhouette picture of a woman."

When object-meaning is present the fields may be less liable to reversal, due either to the persistence of a single object-meaning, or to the persistence of a given field as the bearer of object-character.

P: "The only way that I can hold the figure is to make the figure mean something. This can make a face with a prominent nose. So long as I have that meaning in mind the thing stays."
Z: "By fluctuating between various meanings the black kept stable character."

When object-meaning is present, alternation is accompanied by change of object-meaning.

Changes of dimensionality have been mentioned in connection with alternations of object-character. Such changes may be most striking:

Z: "First looked like a helmet. Striking change when it turned into a bird. The plane of the picture seemed to shift so that the wings became more horizontal. The ground is thin, vaporous, of the character of the sky. The figure stands out. The wings and the posterior part of the body are at right angles in space." "When a particular part determines the perception it seems to be clearer, with greater difference of depth at that point. Alternations between the figure coming out and going back to the level of the ground: seems to come out most clearly when there is a definite meaning."

After protracted observation (30 sec. or more) the *O*s reported unusual experiences of depth. For *B* these were particularly striking:

"Hole now. When it's a hole I get the snow-effect. Now the figure's becoming grey. Now black again. Now black. Now hole. Now grey film, localized way behind. I can see at least two feet back. I can see way, way back in that hole."

These unusual experiences of depth are related to a 'grey haze' that comes with long observation of a stimulus. The clouding-over that appears after steady fixation of a color-spot is a familiar affair. The experience here seems to be similar. *Z* reported:

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tain that in the temporal course of experience a 'meaning' is added on to a previously unmeaningful sensory core. It was very obvious in these experiments that the most primitive experience has a real meaning, though it is not necessarily referred to some familiar object outside its setting. The simplest figure-ground has truly a significance to the subject. Upon more extended observation the perception undergoes change. When this change is such as to produce a report, a naming, as of some familiar object, an object-meaning may be said to be present. The distinction represents merely a particular sort of change in the figure-character of the experience.

"Very marked changes of intensity: grey waves coming over and disappearing. These waves stop before coming to the contour. The contours seem to be blacker than the rest of the figure."

The features of interest in the present context are three in number. The correlation with unusual depth may be understood by reference to the common phenomenon of aerial perspective: due to atmospherical effects, distant objects appear blurred and hazy as compared with near objects. A peculiar fact is that the grey film always stops short of the contours. It would seem that central factors are such as to maintain the contour in its definite form aside from any changes due to retinal adaptation or fatigue. The haze-effect itself periodically comes and goes, without discoverable correlation with alternation of fields.

The experiencing of one or another field as figure is in some degree correlated with the constitution of the stimulus. Rubin is confirmed in the proposition that fields with numerous prongs and lobes are more easily perceived as negative (peripheral-field) figures than are simple fields. Some stimuli are more easily experienced with a particular field as figure. Fixation was reported by all *O*s as one of the determinants in the perception of a given field as figure, though one of the *O*s declared that fixation is not a compulsory factor. This declaration is supported by results of further studies.

P: "Black figure. Now when I fixate the lower white bump the black part may be figure or ground. It is necessary to have something in addition to fixation for the change. What that is I don't know." And again, "It's attention . . . I'm fixating now the white point, and I still see the thing as a black figure, because I attend to the contours at another place than where that white point is. Now when I fixate and attend to the white point the contours become less distinct and less clear, and the white thing looks like the inside of a cave, the black is opening."

One must here leave open the question whether change of fixation occasions a reversal of fields, or whether reversal is prior to a change in fixation.

As suggested above, attention may be one of the important determinants of a given manner of perception of an equivocal stimulus. The reports of the *O*s suggested that the part attended to tends to come out as figure.

One may conclude from the results of the last two series that increase of presentation-time, even as high as 30 sec., occasions no striking change in the figure-ground phenomenon. No new essential features are revealed. Further, one is not able to establish any conditions under which all of the various features of figure-ground experience will in highest development be insured, even 50% of the time. There is evidence that long times, up to a certain point, favor brightness-difference, but militate against perception of the best depth; beyond that point, how-

ever, increased exposure, through adaptation or fatigue, reduces brightness-difference, and gives an experience of most exaggerated depth. The features of heterogeneity, brightness-difference, contour, and shape have been found in all stages to maintain their fundamental position as distinctive of the figure-ground phenomenon.

Summary of Results

The figure-ground phenomenon is a complex affair which on analysis reveals a number of properties. These properties are presented in a series of stages ranging from simplest upwards in the direction of complexity. Certain fundamental properties—heterogeneity, brightness-difference, contour, and shape—are present, in primitive or advanced degree, at all stages of development of the phenomenon. The presence of these fundamental features is essential to the perception of figure and ground.

(1) *Heterogeneity.* For figure-ground perception there must first be present a differentiation of regions within the field of vision. One of these regions is the figural field, the other is the ground. To be a (figural or groundal) field a given visual region must not only be distinct from the remainder of the visual area, but must be within itself homogeneous; at least, the homogeneity of the visual area taken entire must not be greater than the homogeneity of each of the two fields taken separately. Or, what is the same thing or a consequence of this, the figural and groundal fields must each constitute a unit; if the visual area as a whole constitutes a unit, figure-ground differentiation does not appear.

(2) *Brightness-difference.* The intrinsic homogeneity of the separate fields is a function of the distribution of absolute brightness; the heterogeneity of the experience is a function of the difference in brightness of the two fields. A minimum of brightness-difference is necessary for the experience of figure and ground. As brightness-difference increases, the 'goodness' of the experience increases, though at a constantly diminishing rate.

(3) *Contour.* After brightness-difference reaches a certain value there is a perceived 'region of separation' of the two fields. This 'region of separation' is actually a region; it extends over an area. It is grey in quality (for a black and white stimulus). It marks off one field from the other. In earlier stages this separation is not abrupt; it passes gradually from one region to the other. The transition must attain a certain grade before figure-ground experience appears. Below that grade the experience is merely that of a nucleus which is dark in the center and grows gradually lighter toward the periphery; here there is no differentiation of fields. As exposure-time increases above the mini-

mum the separation-mechanism undergoes progressive change, narrowing in its sectional course until there appears in the perception a definite 'step.' This 'step' may have areal properties still (called a 'fuzzy contour'), but contraction continues until it is reduced to a simple line, which in its higher stages loses its dimensional properties and finally is nothing more than the place where the figure ends. This is 'definite contour,' the breadth-less, colorless contour of Rubin. As exposure-time increases from this point, contour increases in its definiteness or sharpness; that is to say, the 'step' becomes a higher 'step.' In the lower stages there are regional differences in the contour; after a certain stage the contour is continuous.

(4) *Shape.* Shape is a property of the figural field. It is a function of the areal distribution of the field, and of the separation-mechanism. Shape occurs before definite contour: a gradual transition is sufficient to outline the shape of the figure. As the contour becomes definite, the 'formed-ness' of the shape becomes greater.

When these fundamental features are minimally present the experience is of the simplest sort. When conditions are such that they occur in more developed state, and additional properties are present, the phenomenon is at a 'higher' stage. Additional properties are minimal localization (or protrusion of the figural field), definite localization, texture, and halo.

(5) *Protrusion of the figural field.* Apparently dependent upon the degree of heterogeneity between the two fields is a protrusion of the figural field. The figure stands forward from the background. This minimal difference in depth-localization of the two fields does not seem to be differentiated from the more impressive, more insistent character of the figural field.

(6) *Definite localization.* In the higher stages of the phenomenon many experiences present a definitely perceived difference in depth-localization of the two fields. Between this and minimal localization occur various intermediate forms. The precise relation between these more definite forms of localization and the minimal type of localization just mentioned is yet obscure. The complete course of the depth-experience with progressive increase of time of exposure has not been worked out, but preliminary results show an initial increase of definiteness of the depth-experience, and then a decline up to the point at which protracted staring brings on retinal changes which result in a decrease of brightness and a coincident increase in depth.

(7) *Texture.* Closely related to a typical form of depth-localization is the texture of figural and groundal fields. The figure, in certain advanced stages of the phenomenon, has a surfaced texture, while the ground is filmy or translucent. The

evidence that appeared is in agreement with previous experiments, which showed relationship between texture and localization. Ordinarily the field that is surfaced has a definite localization, while the localization of the filmy field is indeterminate.

(8) *Halo.* Under certain conditions a halo appears just outside the dark figure as a region which is brighter than the peripheral portion of the ground. Typically, the halo is a part of the ground, though at times it has different localization. It is presumably due to the little-understood phenomenon of simultaneous contrast.

As has been shown, the figure-ground phenomenon is a function of the particular conditions of observation, and changes as the conditions change. With stimulus and setting kept constant, a progressive change in time of exposure and illumination has correlated with it a continuous change in the phenomenon. Certain stages of the phenomenon were selected for intensive study, and may be regarded as reference points in the whole course of development of the phenomenon. These stages are 'simplest' figure-ground experience, 'good' figure-ground experience, and a highly-developed stage that appeared in the quest of an experience that could be called 'perfect.'

(*1*) *Simplest figure-ground.* If within the field of vision there appear two intrinsically homogeneous regions with a degree of heterogeneity sufficient to give a differentiation into what can be called two fields, if there is, for a black and white stimulus, a minimal difference of brightness between the two fields, if there is a certain grade of separation between the two fields, and if one of these fields has an areal distribution of a certain perceptual value, then figure-ground experience is present in its simplest form. Liminal conditions for this stage are about 10 σ.

(*2*) *Good figure-ground.* When the fundamental properties of 'simplest' figure-ground experience are present in more highly developed state, the brightness-difference has a greater value, the separation-mechanism is a 'step' continuously bounding the figure, and the shape is fairly well 'formed;' and when in addition the figural field protrudes in the field, the experience can be termed a 'good' figure-ground experience. Definite separation of the two fields in depth may be present, but is not essential. Liminal conditions for this experience are 11-17 σ.

(*3*) *'Perfect' figure-ground.* As exposure-time increases, the fundamental properties of figure-ground experience attain for the most part greater definiteness, but there were found no liminal conditions under which an experience arises that can be called 'perfect.' With longer times of exposure texture-differences between figure and ground are more likely to be demonstrated. With increase of exposure-time the various properties

do not develop at the same rate; one, depth, may even vary inversely along part of the temporal course. This would seem to preclude the appearance of an experience containing all the properties of figure-ground experience in most highly developed state.

Conclusion

The share of consideration that has of late been accorded the phenomenon of figure and ground is due in large measure to the assimilation by *Gestaltpsychologie* of the experimental work of Rubin, and the status of this figure-ground concept in the theory as a fundamental type of mental structure. As such a type the figure-ground experience has been a focal point of many systematic issues, for it involves the question of a fundamental analysis of simple visually perceived form into figural and groundal components. Moreover, since figure-perception is said to involve thing-perception and the appreciation of wholes, it is plain that the experience may be crucial in the understanding of the problem of objectification and meaning. This theoretical significance of figure and ground in the visual perception of form was apparent even at the time of Rubin's study, but recently the situation has grown much more critical because of the conflict between *Gestalt* theory and traditional psychological method. If the figure-ground experience is, as held by the *Gestalt* group, the simplest perceptual type, its analysis and conditions should give some cue to the general problem of perception.

This analysis, as conducted in the present setting, was by way of controlled variation of the time of exposure of simple, equivocal stimuli, with the maintenance of relatively constant attitude and conditions of observation, so that any change in the phenomenon might be regarded as a function of a simple variable—time (and a quantity dependent upon time, intensity of illumination). Changes do in fact appear: as the time of presentation increases from very brief exposures, the experience changes in the direction of definiteness and complexity.

At the lowest level of the figure-ground experience few features appear, under the conditions just outlined, and these in but poorly developed state. The figure-ground experience of the very simplest sort shows as distinguishing features (1) a given degree of heterogeneity of the visual field, in which two regions are discriminated, (2) a certain stage of brightness-difference between these regions, (3) a liminal value to the demarcation, or contour, separating the fields, and (4) such an areal distribution of the visual qualities that one field, the figure, has a simple, perceived shape. The separation-mechanism at this stage is no definite, well-marked affair, but a region of

transition which may vary in breadth from one experience to another, and in different portions of the same experience. At a somewhat more advanced stage of the phenomenon definite contour begins to occur, showing itself in a limited portion of the experience as an abrupt transition from one field to the other.

This abrupt gradation is later extended so as to encircle the figural field as a continuous or complete, definite contour. Its development is accompanied by changes in other fundamental features: the brightness-difference is much greater, the degree of heterogeneity is increased, and the shape has taken on more and more 'form.' Moreover, the figural field presents a primitive type of localization, a protrusion in the visual field. This stage may be called 'good' figure-ground experience. Definite localization of the figural field may be present at this time, but shows itself more prominently somewhat later.

With increase of exposure-time the various features are presented for the most part in more highly developed state, though there seems to be operating a law of diminishing returns: at the remote end of the developmental course a further increase of presentation-time has little effect. Depth-localization is the least uniform feature. It appears in a variety of forms and degrees, and of the factors mentioned above it is least subject to control. A halo-experience comes in at a more primitive stage of the phenomenon, but is not an essential feature of figure-ground experience. Nor is texture, which, as it tends to appear at comparatively advanced stages, may be classed among the 'higher' properties.

No compulsory conditions were found for a 'perfect' experience of figure and ground. With increase of exposure-time the various isolated factors do not develop at the same rate. In fact, time may be increased to a point at which adaptive changes cause a reversal in the direction of development of brightness-difference by reduction of visual intensity. No new features were found to appear with increase of exposure beyond the brief times involved in 'simple' and in 'good' figure-ground experience, so that from these data one is led to conclude that the essential features of the figure-ground phenomenon have been revealed.

The nature of the experience of figure and ground depends upon many things. An analysis may be effected by variation of one phase of the situation while other conditions are constant, yielding a series of experiences correlative to the variable, time. It is the whole correlated series that is to be regarded as *the figure-ground phenomenon*. A given figure-ground experience, then, is but one of many possible stages of the complex perceptual phenomenon, and its precise nature is a function of the various components of the situation in their entirety.

THE INFLUENCE OF MUSCULAR TENSION ON THE EFFICIENCY OF MENTAL WORK[1]

By Arthur Gilbert Bills, University of Minnesota

Problem

It is commonly assumed, with some experimental justification, that intense mental activity is accompanied by increased muscular tension. The present study was undertaken to determine whether such increased muscular tension exerts any influence upon the efficiency of mental work, or whether it must be considered as merely an overflow phenomenon without functional significance. It was hoped that the investigation might throw some light upon the nature of *effort*, how it is exerted, and how it reacts upon mental efficiency.

Historical Summary

The direct evidence thus far is limited to one side of the problem only, that of demonstrating that increased muscular tension accompanies mental work. The other side of the problem, the question as to what functional relationship this increased tension bears to mental efficiency, has not yet been attacked directly. The evidence which exists is indirect.

With regard to the first question, the preponderance of evidence is positive. On the negative side, Loeb (1888) found that pressure on a dynamometer tended to slacken when accompanied by mental activity, and concluded that mental effort lessened muscular effort. His results were confirmed by Lehmann (1900) with the ergograph. R. MacDougall (1896) measuring the contraction of the index finger during concentrated attention, found a reduction in tonicity.

On the positive side, Lombard (1887) found that the knee-jerk was enhanced by accompanying mental activity, and his findings were very recently confirmed by Tuttle (1924). Dresslar (1891) got similar results with voluntary tapping. Féré (1889) found that subjects would increase their maximum squeeze on the dynamometer by one-fourth, when mental activity was induced. Among others, Mosso (1894) obtained a heightened ergographic curve from a subject engaged in strenuous mental activity. The most extensive work has been done by Golla (1921) who measured the tonicity of the various body muscles during mental effort by the use of extremely delicate apparatus, and invariably obtained heightened curves.

As has been said, the evidence bearing on the second half of the problem, the influence of this increased tension on mental efficiency, is mainly indirect. Several studies tend to show that increased *attention* tends to increase the intensity of sensation. In the field of vision, W. McDougall (1904) found that when successive flashes were presented 1 sec. apart, either could be made brighter by voluntarily setting the attention. One of the most recent studies is that of Newhall (1923), who found that attention to cutaneous and visual stimuli increased their intensity. The difficulty is that such studies as these employ a subjective definition and measure of attention, and do not necessarily imply increased muscular tension. That muscular tension was probably involved, however, is evident from studies such as those of Jacobson, Breese, and Miller.

[1] From the Psychological Laboratory of the University of Chicago.

Jacobson (1912), in a study upon the influence of distracting stimuli in inhibiting sound sensations, found that the effectiveness of the distractor depended on the amount of attentive effort which subjects reported to be given to the distractor. Moreover the influence of the inhibiting agency could be neutralized by voluntary attention, and when the distractor caused the subject to increase this effort of attention to the sound, the sound seemed even stronger than when it occurred with normal attention, without distraction. He then tested the influence exerted on the intensity of the sensation by the abandonment of all attentive effort. This required teaching the subjects how to abandon attentive effort. From their introspections and objective behavior, he discovered that attentive effort involved tense muscles. "In attentive effort," says he, "the observer's brows might be wrinkled, his mouth compressed, the muscles of his trunk and limbs alert and contracted." By requesting the subjects to eliminate these tense attitudes, he succeeded in securing a high degree of effortlessness, with resulting decrease in sensation intensity. Upon reversing the procedure and allowing the subject to exert strong muscular effort to attend to the sounds as distractors, he succeeded in getting complete inhibition of odors.

Breese (1899), in working with the phenomenon of retinal rivalry, discovered that contraction of the arms and legs of either side of the body, and the maintenance of this rigid tension during the experiment, tended to lengthen the periods of visibility of the respective fields.

Miller (1925) found in administering electric shocks to the arms of her subjects, that, if they relaxed the body musculature, the intensity of the subjective experience of the shock was greatly reduced or even entirely removed.

Many studies demonstrate the influence of attention on the speed of reaction. Cassel and Dallenbach (1918) found that shortness of reaction time is correlated with attention. Others find that the interval most favorable for preparing and holding the attentional attitude by a warning signal is most conducive to short reaction times. The shortening of reaction time with the 'motor' as opposed to the 'sensory' attitude, originally noticed by L. Lange (1888) working under Wundt, may be due to the fact that the motor set involved more muscular tension. This seems to be borne out by the work of Judd (1905) and of Williams (1914) in analyzing the difference in preparatory muscular adjustment involved in the two types of set.

The discovery by many investigators that stimuli used as distractors often act as facilitators may be explained by the fact that greater muscular tension is called forth by distraction. Morgan (1916) reports an improvement in the speed and efficiency of mental work as a result of introducing sensory distractors. He explains it by the fact that his subjects put forth greater effort under distraction. This effort involved muscular tension, shown by the greater pressure on the reacting key, and by muscular activity of the speech organs, etc. Conversely, speed of reaction is reduced by relaxation of the body musculature, as is shown by the work of Miller (1925).

The broader question of the relation of muscular tension to 'purpose,' and the 'problem idea' has been given admirable theoretical treatment by Washburn (1916). "The motor innervations underlying the consciousness of effort," she says, "are not mere accompaniments of directed thought, but an essential part of the cause of directed thought."

It is entirely probable that the results which were secured by W. McDougall (1919) in his investigation of the influence of the 'conative' attitude upon the speed of learning and efficiency of retention of memorized material, were due to the fact that his 'conative' attitude involved greater muscular tonicity than the control condition. At least such studies could be made quantitatively more clear-cut if the variable condition could be

defined in terms of amount of muscular tension rather than by such a vague factor as 'conative tendency,' or the absence of it.

The foregoing historical survey demonstrates the need of a quantitative and qualitative study of the influence of muscular tension on the efficiency of mental activity.

Analysis of Problem

Will an increase in the total amount of muscular tension in the body increase mental efficiency? It is impossible to measure the total amount of tension in the body. But it is possible voluntarily to increase this amount by a definite increment and to note the effect of this increase on mental output, as contrasted with the output under the normal condition. The problem is complicated by the following factors:—

First, in evaluating the efficiency of mental work by the nature of the output, certain criteria of excellence must be chosen. The speed with which the work is done constitutes an important measure of efficiency. Yet speed is meaningless unless taken in conjunction with quality. This, in the case of learning problems, refers to the thoroughness with which the material is assimilated, and may be measured by the amount recalled and the saving in relearning over the original learning time. In other mental tasks it usually refers to the accuracy of the results. Hence our problem is to determine just how tension influences each one of these factors individually.

Secondly, since the largest amount of tension and random muscular activity has been observed to accompany the early stages of learning, the question arises whether after practice in any type of mental work, the initial benefit from muscular tension will tend to decrease, until tension becomes no longer effective, *i.e.* in other words, what effect will practice have?

Finally, how will muscular tension influence the general decrement from continuous mental work? Since it involves fatigue of muscle tissue, will it add to or decrease the mental fatigue?

The problem may therefore be restated as follows: (1) Will an increase in muscular tension beyond the normal result in an increase in the efficiency of mental work? (2) Will this effect increase or decrease with practice? (3) Will it increase or decrease with the onset of fatigue? (4) Will the results differ for different criteria of mental efficiency?

Apparatus and Methods

Four distinct experiments were performed. They differed from one another in the type of mental work used, the work instructions, and the criteria of efficiency measured. But throughout all of them, the method of attack was similar. It consisted in securing a large number of measures of mental output from every *S* under two different conditions: one where the

normal amount of muscular tension was used; the other where it was increased by a definite and constant amount. Hereafter these two conditions will be referred to respectively as *normal* and *tension*. They were always given alternately, so that practice and fatigue effects were equalized for both in the averages. Additional statistical validity was secured by the order of arrangement of individuals.

The *S*s sat in customary posture before a table on which was exposed the material to be used. If memory material, it was presented through a Chicago memory-drum. Otherwise it was exposed on a tilted standard at a convenient distance for clear vision. The only difference between the normal and tension conditions was that, under the latter, the *S*s grasped, and squeezed continuously at a constant pressure, spring dynamometers, one for each hand. The strength of the dynamometers had been reduced two-thirds by inserting weaker springs. An optimal degree of pressure was determined for each *S* in a preliminary series, and then adhered to throughout. Experiment I differed from the others in being exploratory. Hence both constant and intermittent tension were used, and the amount was allowed to vary. Kymographic records were kept by connecting the dynamometer needle with a recording device, and the resulting records were compared with the performance. It was found that constant tension was as satisfactory as intermittent tension, and was less disagreeable. Moreover, practice soon enabled the *S*s to maintain a constant amount without prompting. Hence the recording device was discarded in later experiments. The weakening of the springs, and the use of two dynamometers instead of one, were changes made after the first experiment.

Experiment I

In Experiment I the mental work consisted of *learning nonsense syllables*. Nine *S*s, 4 women and 5 men, all advanced college students, took part. They were kept in ignorance as to the purpose of the experiment. Every individual reported twice daily, once to learn the list of nonsense syllables, and again 3 hrs. later to recall and relearn it. The two work conditions were employed on alternate days so that, of the 20 lists which every *S* learned during the 20 days of the experiment, 10 were learned under the tension and 10 under the normal condition. Half the *S*s began on tension, half on normal. The regular series was preceded by 2 days of practice under both conditions to familiarize the *S*s with the procedure.

Carefully selected lists of comparable difficulty were used. Every list contained 9 syllables of 3 letters each, exposed one at a time, and learned by the anticipation method. That is, after the list had been presented once, during subsequent presentations *S* recited every syllable he could recall, just before it appeared. When he could anticipate the entire list correctly, it was considered learned. Record was kept of the number of complete presentations of the list necessary to learn it, and of the syllables correctly anticipated during every presentation. Since each syllable was exposed just 2 sec., and the lists contained 9 syllables, the actual learning time can be computed, for a given list, by multiplying the number of presentations by 18.

Record was kept of the number of syllables recalled on the first presentation, and of the number of presentations necessary to relearn the list. By comparing this with the original learning time, the saving in relearning could be computed thus:

$$\text{Saving score} = 100 - \left(\frac{\text{presentations necessary to relearn}}{\text{presentations necessary to learn}}\right) \times 100.$$

This gives us the following measures of learning efficiency: (1) learning time; (2) syllables recalled; (3) percent saved in relearning.

A comparison of the learning records for successive days displays a well marked practice curve for both conditions, from which we can judge whether the relative effectiveness of the two contrasted work conditions is differently influenced by practice.

Results of Experiment I: (*A*) *Absolute ratios between 'tension' and 'normal' scores.* By all three criteria of learning efficiency, 'tension' is shown to be more efficient as a work condition than 'normal.' The average learning time under the normal condition, as measured in repetitions, is 9.8 repetitions per list, whereas under tension it is only 8.6, a decrease of 1.2 repetitions per list, or approximately 13%. The probable error of this difference is 0.27, which insures its statistical significance for all practical purposes. On the whole, then, we may say that our *S*s learned more rapidly under increased muscular tension.

This result would have little significance, however, if it were coupled with a compensating loss in the efficiency with which the material was assimilated, as shown by the recall and saving criteria. Yet, as a matter of fact, the tendency was to recall a greater number of syllables in the case of the lists learned under tension. The average recall for lists learned under 'normal' is 3.9 syllables; for lists learned under 'tension' it is 4.4 syllables, an increase of 0.5, or nearly 13%. In this case the probable error of the difference is not so low, being 0.198, or about ⅔ of the difference, which makes the chances more than 5 to 1 that a new pair of measures would show a difference in favor of 'tension.' Again, when we compute the saving scores, we find that the average saving for lists learned under 'tension' is 64.4%, while for those learned under 'normal,' it is only 60.6%, a difference of 3.8, or about 6% in favor of 'tension.' The raw relearning scores show a more marked difference. The average number of repetitions required to relearn lists learned under 'normal' is 4.9, whereas for those learned under 'tension' it is only 3.7, a decrease of 1.2 repetitions, or about 32%.

A comparison of the learning records, under the two contrasted conditions, as to the proportion of correct to false responses during the actual learning, throws light on the cause for the better retention of material learned under 'tension.' The ratio of correct responses to total presentations gives us what might be called an *overlearning score.* The more overlearning a syllable receives, the better it will be retained, we assume. The

TABLE I

Absolute Ratios Between Normal and Tension Scores

S	LEARNING TIME: No. Repetitions				RECALL: No. Syllables Recalled				RELEARNING TIME: No. Repetitions				SAVING: % Saved in Relearning				OVERLEARNING: Ratio Between Presentations and Correct Responses			
	Tension	Normal	More with Norm.	% More with Norm.	Tension	Normal	More with Tens.	% More with Tens.	Tension	Normal	More with Norm.	% More with Norm.	Tension	Normal	More with Tens.	% More with Tens.	Tension	Normal	More with Tens.	% More with Tens.
Al	7.1	6.7	−0.4	−6.0	5.2	5.1	0.1	2.0	3.2	2.6	−0.6	23.0	65.0	72.5	−7.5	10.3	71.3	71.2	+0.1	0.14
Fi	10.1	10.0	−0.1	−1.0	5.7	5.2	0.5	9.6	3.7	4.0	0.3	7.5	74.6	70.1	4.5	6.4	62.4	60.5	1.9	3.1
Fr	14.1	11.7	−2.4	−21.0	4.1	2.9	1.2	41.4	4.7	6.5	1.8	28.0	71.6	51.5	20.0	38.8	63.0	58.8	4.2	7.1
He	7.8	7.9	0.1	1.3	4.0	1.9	2.1	111.	3.8	6.7	2.9	43.3	60.9	37.1	23.8	64.2	70.4	65.6	4.8	7.3
Li	5.5	7.5	2.0	26.6	2.5	3.6	−1.1	30.5	5.1	3.7	−1.4	37.8	39.0	59.0	20.0	34.0	72.0	73.4	−1.4	−2.0
Mo	9.4	12.4	3.0	24.0	0.5	2.8	−2.3	82.0	5.3	9.3	4.0	43.0	47.4	47.8	−0.4	−0.8	69.7	63.6	6.1	9.6
Pi	7.0	12.0	5.0	42.0	6.5	5.5	1.0	18.2	2.3	3.5	1.2	34.0	77.3	72.2	5.1	7.0	76.7	66.4	10.3	15.5
Sh	6.9	7.0	0.1	1.4	5.6	4.7	0.9	19.2	2.3	3.1	0.8	26.0	70.0	65.0	5.0	7.7	65.6	62.5	3.0	4.8
Te	9.2	12.9	3.7	28.0	5.3	3.9	1.4	36.0	3.4	4.2	0.8	19.0	74.1	70.3	3.8	5.4	56.9	51.4	5.5	10.7
Av.	8.6	9.8	1.2	14.0	4.4	3.9	0.5	13.0	3.7	4.9	1.2	32.0	64.4	60.6	3.8	6.0	67.5	63.7	3.8	6.0
N = 90	P.E. = .27				P.E. = .20															

'tension' condition shows an average overlearning score of 67.5, or 6% more than that under 'normal.' Is there not, then, justification for concluding that a larger proportion of the syllables are learned at once, under tension, and hence receive more overlearning? Under the normal condition, on the other hand, they are learned later in the game, and hence suffer from lack of repetition, and are sooner forgotten.

Table I shows the relative efficiency of every *S* under the two contrasted conditions, as measured by each of the criteria tested. It will be noticed that 6 *S*s out of the 9, or ⅔ of the group, learned more quickly under 'tension,' and that of the 3 who did not, every one compensated for this fact by recalling more under 'tension.' On the other hand, 2 of those who learned more quickly under 'tension' compensated for this gain by a loss in recall efficiency. Since no one criterion is more valid than another, the best way to get at the true measure of efficiency is to determine how many of the *S*s excelled in every criterion under tension, and how many of the rest excelled in the majority of the criteria. We find that 4 *S*s, He, Pi, Sh, and Te, excelled in all criteria under tension; and that, of the remaining, 3 excelled in the majority of criteria. Hence, on the whole, tension was a more efficient condition for more than ⅔ of the *S*s in this experiment.

(*B*) *Influence of practice on the relative efficiency of 'tension' and 'normal.'* A comparison of the practice curves for the two conditions, when *learning time* is the criterion, shows that the decrease in time due to practice is somewhat greater for tension than it is for the normal condition. With *recall* and *saving* as criteria, however, the opposite is true; the normal condition shows an increase in amount recalled as a result of practice, whereas the tension condition shows an actual decrease, even though this decrease is not enough to incline the balance for the whole experiment. As previously mentioned, the absolute averages show 'tension' to be the more efficient. Evidently the different criteria are differently affected by practice, and consequently require separate analysis.

(1) *Practice and learning time.* Table II shows how practice influences the difference between the 'normal' and 'tension' conditions in speed of learning.

TABLE II

Influence of Practice on Learning Time: Average Number of Repetitions

NORMAL			TENSION			DIFFERENCE	
First Half	Second Half	Gain	First Half	Second Half	Gain	Greater Gain With Tension	% Greater
10.6	9.0	1.6	9.6	7.5	2.1	0.5	31

It gives, for both conditions, the average learning time for the first 5 lists and for the last 5 lists and the difference, showing the decrease in time due

to practice. The last two columns of the table show how much greater this decrease, or practice effect, is under tension. The averages for all *Ss* show a difference of 31% in favor of 'tension.'

Graph I gives a clearer picture of the difference in trend, showing as it does the day-to-day changes. It also enables one to compare the initial and final day's records. The 'tension' curve begins at a higher level, but soon drops well below the 'normal' curve. We may conclude, then, that the increase in speed due to practice is somewhat greater with tension than under normal conditions.

(2) *Practice and recall and saving.* Table III gives data similar to that of Table II, the criteria this time being the average recall and saving for the first 5 and for the last 5 lists learned. Here the opposite tendency is noted in both cases: under the normal condition, the recall and saving scores show a very slight tendency to improve with practice, whereas with tension there is a falling off in initial efficiency, although this never reaches the low point at which the 'normal' scores started.

TABLE III

Influence of Practice on Recall and Saving

AVERAGE	NORMAL			TENSION			DIFFERENCE	
	First Half	Second Half	Gain	First Half	Second Half	Gain	Smaller Gain with Tension	% Smaller
No. Syllables recalled	3.8	4.2	0.4	5.0	3.8	−1.2	1.6	400
% Saved in relearning	58.6	62.6	4.0	70.3	58.5	−11.8	15.8	400

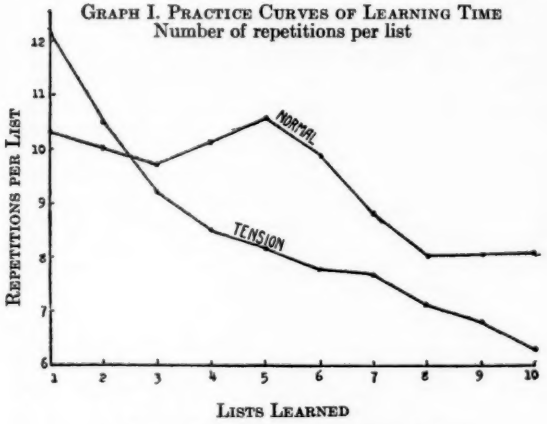
Evidently, for these criteria, tension is no help to efficiency after the habit has once been well fixed. Thereafter it merely increases speed without affecting the amount recalled, or the saving in relearning.

Graphs II and III show the same tendency. The 'normal' curves take a more or less horizontal course, to which the 'tension' curves drop eventually. Thereafter they tend to travel along together at the same level. There is some evidence, however, that, if the curves were prolonged further, the 'tension' curve would continue at a slightly higher level.

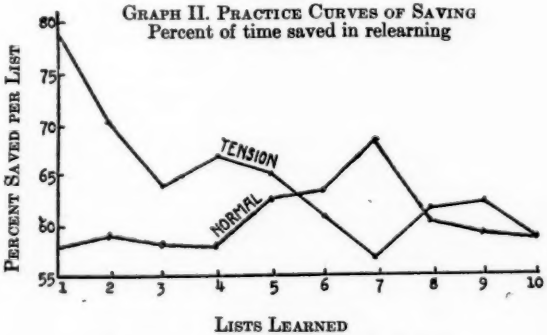
Summary. The purpose of Experiment I was to determine the influence of muscular tension on the efficiency of learning nonsense syllables. Record was kept of the learning time in number of repetitions per list, of the number of syllables recalled, and of the saving in relearning. *By every criterion, tension proved to be the more efficient condition for the majority of subjects.*

The influence of practice on the relative efficiency of the two conditions was then determined by plotting the day-to-day scores, and by comparing the scores for the first 5 lists with the scores for the last 5. It was found that the tension condition gained in relative efficiency with practice, where learning time was the criterion, but that for the other criteria, the initial difference in favor of tension tended to disappear, so that the two conditions became more nearly equal in efficiency.

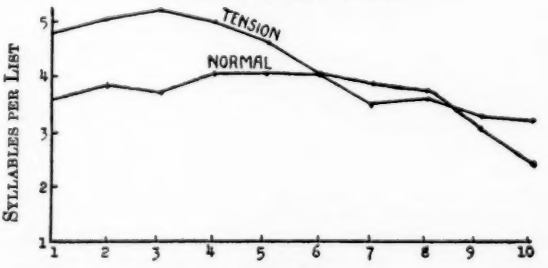
GRAPH I. PRACTICE CURVES OF LEARNING TIME
Number of repetitions per list



GRAPH II. PRACTICE CURVES OF SAVING
Percent of time saved in relearning



GRAPH III. PRACTICE CURVES OF RECALL
Number of syllables recalled



Experiment II

In Experiment II, the mental work consisted of *learning paired associates*. The 10 *S*s used were all graduate students of considerable training, 3 of them women, the rest men. The experiment differed from the preceding one in the following points: (1) Real words, selected at random, were substituted for nonsense syllables. (2) The words were arranged in lists of 10 pairs, and learned as paired associates instead of by the anticipation method. That is, the second word of each pair was recalled on presentation of the first. Tension was not used during recall. (3) The lists were presented only once, every pair being exposed just 3 sec. by means of a Chicago memory drum, and recall followed immediately.

The purpose of using sense words was to determine whether the results of the previous study would be altered bya change from pure rote to semi-logical learning. The paired associate method sets up simple horizontal bonds, and hence lends itself better to quantitative analysis where learning is incomplete. Most important of all was the third change. In the previous experiment we were dealing with two dependent variables, *time* and *recall*. This made it impossible to judge how tension and relaxation would affect *recall* alone, when not complicated by variations in learning time. Hence, in Experiment II, time was held constant, at 30 sec. per list, under both conditions for all *S*s.

Practice effect was equalized for the two conditions, in the following way: Every *S* learned 4 lists a day, the first and third under one condition, the second and fourth under the other. If, on Day 1, he learned lists 1 and 3 under tension, then on Day 2, he learned lists 2 and 4 under tension, etc. Moreover, half the *S*s began with tension and the other half began with relaxation. Thus, by this counter balanced order, both conditions were equally favored as to position. In all, 50 lists were learned under tension, and 50 under the normal condition. The regular series was preceded by two practice sittings, to familiarize the *S*s with the two conditions, to ascertain their optimal degree of tension, and to train them in maintaining a constant tension throughout the learning of a list.

As had been explained previously, the set-up for the tension condition was simplified in this and later experiments. Two dynamometers of ½ strength were employed, one for each hand. An optimal tension was determined for every *S*, by determining the amount of pressure he could maintain without annoyance. This depended, of course, on his strength. He was then instructed to use this amount during each 'tension' exercise, and the experimenter made sure that the degree was constant by watching the dial needle. Although the *S*s were invariably prompted before beginning, it was extremely rare for an *S* to need any reminder during a sitting. A habit or set was established during the practice sittings and maintained unconsciously.

Record was kept of the number of words correctly recalled. As in the previous experiment, a comparison of the performances of successive days displays a marked practice curve for both conditions, which enables us to judge whether the two conditions respond differently to practice.

Results of Experiment II: (A) Absolute ratios between 'tension' and 'normal' scores. A glance at the average results for all *S*s in Table IV shows that without exception every individual recalled more words under tension than under relaxation. On the average, 13% more words were recalled under tension; and

the extreme case is that of Pa, who recalled 50% more material under tension than under the normal condition. The statistical significance of these differences in favor of tension is shown by the last column, which gives the number of times the difference exceeds its probable error. In the case of the general averages, this is over 6 times.

TABLE IV

Number of Words Recalled Per List

S	Tension	Normal	M_1-M_2	P.E.M_1-M_2	Diff. exceeds P.E.	% More recalled under Tension
Bn	5.0	4.4	.6	.31	2	14
Be	7.3	6.4	.9	.24	3.8	14
Co	8.7	8.3	.4	.17	2.3	5
Dr	4.1	3.2	.9	.14	6.4	28
Ga	3.7	3.2	.5	.18	2.8	16
Hi	8.9	8.4	.5	.14	3.6	6
Ho	5.6	4.8	.8	.21	3.8	16
Pa	6.0	4.0	2.0	.28	7.1	50
Ws	6.4	6.2	.2	.31	0.7	3
Wh	4.5	3.9	.6	.22	2.8	15
Av.	6.0	5.3	.7	.107	6.5	13

TABLE V

Distribution of Recall Scores

S	Number of Times More Words Recalled Under		
	Tension	Normal	Equal
Bn	23	14	13
Be	32	12	6
Co	26	15	9
Dr	31	9	10
Ga	23	17	10
Hi	21	10	19
Ho	27	13	10
Pa	38	4	8
Ws	18	20	12
Wh	28	12	10
Av.	26.7	12.6	10.7

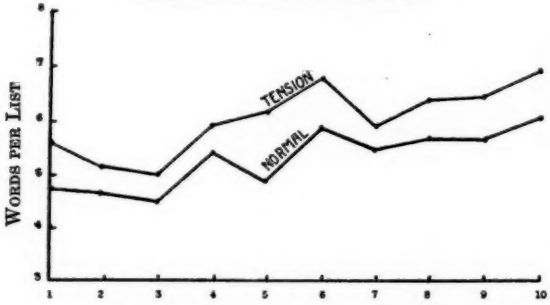
Table V shows, for every *S*, the day to day consistency with which tension gave superior results. If we place the 50 lists learned under 'tension' and the 50 learned under 'normal' side by side, we find that every *S* except one got superior results much more often under tension than he did under normal conditions. In fact, the average for all *S*s shows that tension gave superior results more than twice as often as the normal condition. The extreme case is that of subject Pa, who displayed superior efficiency more than 9 times as often under tension.

Learning under tension is thus shown to be more efficient for meaningful material, when time is held constant and amount

retained is the criterion of measurement. We may conclude, then, that the changed conditions of this experiment did not alter the fact established in the previous experiment, namely, that tension gives a higher absolute level of efficiency than the normal condition.

(*B*) *Influence of Practice.* It is in the practice curves obtained by plotting the day-to-day recall scores under each condition that we find a decided contrast between this and the preceding experiment. In the former study, whereas for speed of learning the initial advantage in favor of tension increased with practice, the recall ratio took the opposite course, so that finally

GRAPH IV. PRACTICE CURVES OF RECALL
Number of words recalled per list



UNITS OF PRACTICE (groups of 5 lists)

the tension and normal levels were about equal. In the present experiment, on the contrary, practice does not change the relative levels of efficiency displayed under the two conditions. Tension gives about 20% more recall than normal, from the first day, and continues to hold its advantage throughout the series.

In Graph IV, each point on the curves represents an average of 5 sittings for 10 subjects, or 50 measures. This gives us 10 reliable practice units for each condition, from which to judge the trend. It is evident that the higher level gained with tension is consistently maintained. Does this not suggest that probably the tendency in Experiment I for the tension curves of recall to lose their initial advantage was due to the fact that learning time was not held constant?

Summary. The purpose of Experiment II was to determine whether a change from nonsense to sense material would alter the relative levels of tension and normal efficiency, and whether, if the learning time were held constant, we should still get what we did in Experiment I, namely, a shift in favor of the normal

condition as a result of practice in recall. The results prove that tension is more efficient as a learning condition for sense as well as nonsense material, and that, with a constant learning time, practice does not alter this situation.

Experiment III

Learning is a very special kind of mental work. This research would be incomplete if our tests were not also applied to other forms of mental activity involving more routine intellectual operations. The last two studies will therefore be devoted to non-learning functions, where the operations involved are assumed to be fairly habitual ones. This not only gives us a more comprehensive treatment of the problem, but at the same time enables us to introduce a new criterion of efficiency, that of accuracy. What is the influence of tension on accuracy of performance?

In Experiment III the mental work consisted of adding columns of 20 digits. Since the average time necessary for the adding of one of these columns was 20 sec., it was possible for the *S*s to add 12 separate columns in one session. Every *S* reported for 10 sessions, 2 a week for 5 weeks, so 120 distinct measures were obtained from each. Eleven *S*s, graduate students in psychology, participated in these experiments.

The columns of digits were constructed as to be comparable in difficulty, yet each different in order from all the others. Zeros and ones were discarded. All the 8 remaining digits appeared in every column. The columns to be added appeared on typewritten cards placed on a raised standard at convenient distance from *S* for clear vision. When the sum was obtained, it was reported orally and checked right or wrong by *E*. The time in seconds was also taken by means of a stop-watch. In this manner a record was obtained regarding two criteria of efficiency, *speed and accuracy of performance.*

The two conditions, tension and normal, were alternated from list to list, and the other additional checks that we described in Experiment II were also used. The directions were given orally. After placing the card containing the figures in front of *S*, *E* said, "Tension; column one; ready; go;" or "Normal; column one; ready; go;" depending upon the condition to be used. At the word "tension," *S* grasped the dynamometers and squeezed them with the appropriate pressure. At the next command he fixated the top of the column, and at the signal "go," added as rapidly and as accurately as possible. If the word were "normal," however, *S* folded his hands in his lap, relaxed, and then the procedure was the same.

Instructions were given at a fixed rhythm and an attempt was made to give them uniformly in the same tone of voice and bearing. Between every two additions, *S* turned his eyes to the window and rested them for 10 sec. This respite enabled *E* to record the time and accuracy.

Since every *S*'s daily record contains 6 successive measures under each condition, it is possible to plot regular work curves for both 'tension' and 'normal,' showing what influence fatigue has upon the relative levels of efficiency of these contrasted conditions. Inasmuch as there were 10 successive work-sessions in the total experiment for both conditions, practice curves can be plotted which will indicate whether tension loses its effectiveness as the task performed becomes automatic.

Results of Experiment III: (*A*) *Absolute ratios between 'tension' and 'normal' scores.* The average time required for all the *S*s to add a column of 20 digits under tension was 23.3 sec., under the normal condition, 25.7 sec.; a difference of 2.4 sec. in favor of tension. In other words it took on the average 10% less time to add a column when tension was used. That this is a reliable difference is shown by the fact that it exceeds its

TABLE VI
Average Number of Seconds per Example

S	Tension	Normal	M_1-M_2	P.E. M_1-M_2	Diff. times P.E.	% Less Time under Tension
Pa	21.4	28.1	6.7	.69	10	24
Wa	14.2	16.9	2.7	.334	8	16
Sh	10.1	12.1	2.0	.25	8	17
Bi	16.7	19.9	3.2	.25	12	17
Br	26.4	28.0	1.6	.613	3	6
Th	19.4	19.7	.3	.48	1	2
Wi	29.1	31.1	2.0	.7	3	7
Gn	28.0	29.0	1.0	.68	1.6	4
Lo	27.7	28.7	1.0	.47	2	4
Yu	31.5	34.0	2.5	.68	4	7
Ho	32.3	34.9	2.6	.92	3	8
Av.	23.3	25.7	2.4	.321	7.5	10

N = 660

TABLE VII
Distribution of Time Scores

S	Number of Times Greater Speed is Gained With		
	Tension	Normal	Equal
Pa	50	8	2
Wa	50	7	3
Sh	44	8	8
Bi	52	2	6
Br	34	24	2
Th	31	25	4
Wi	36	21	3
Gn	34	24	2
Lo	34	23	3
Yu	37	21	2
Ho	35	24	1
Av.	39.7	17	3.3

probable error by 7.5 times. A glance at the individual records in Table VI shows that there were no exceptions to this rule. But in some cases the difference was so slight as not to be statistically significant. Hence we have indicated in each case the number of times the difference exceeds its probable error. Two cases are on the borderline, and three fall below the statistical requirements.

Regarding the consistency with which tension excelled, we have evidence in Table VII, which shows the proportion of times that tension gave superior results, in the case of every *S*.

On the average, out of 100 mean scores, 67% show better results with tension, 28% with normal, and 5% are tied. In the extreme case, that of Bi, tension gave superior results 87% of the time, while normal excelled only 3%. The least successful case, that of Th, gives superior records for tension 10% more times than for normal. For the speed criterion, then, the results point unmistakably to the superiority of the tension condition.

An analysis of the *accuracy* records is given in Table VIII.

TABLE VIII

ACCURACY

Absolute Ratio Between Normal and Tension Scores

S	Normal	Tension	Gain	% Gain
Pa	5.4	5.6	.2	4
Wa	5.4	5.8	.4	7
Sh	4.5	5.3	.8	17
Bi	5.8	5.9	.1	2
Br	5.2	5.4	.2	4
Th	4.3	5.2	.9	21
Wi	5.5	5.9	.4	7
Gn	4.7	5.1	.4	9
Lo	5.9	5.8	−.1	− 2
Yu	4.3	4.3	0	0
Ho	5.3	5.0	−.3	− 6
Average	5.1	5.4	.3	6

N = 660 P.E. of Difference = .026

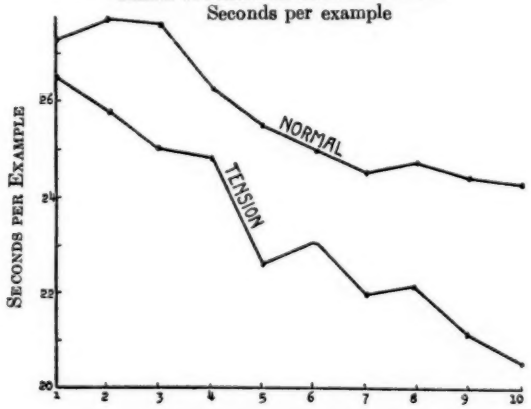
The average daily number of columns correctly added under tension was, for all *S*s, 5.4, as against 5.1 under the normal condition. This means that tension gave 6% greater accuracy; not a large difference but a significant one, since it is over 12 times its probable error. Th, who showed the least difference in speed, showed the most in accuracy, in favor of tension. In general, though, speed and accuracy were neither positively nor negatively correlated, as is shown by applying the Spearman Rank Method to the last columns of Tables VI and VIII. The ρ formula gives a correlation coefficient of $-.16 \pm .19$.

(*B*) *Influence of practice.* In spite of the fact that such a mental function as adding figures is fairly automatic to start with, it is capable of being considerably improved with intensive practice such as that afforded by the present study. The practice curves for the two conditions show a decided drop in the amount of *time* per column between the first and last sessions of the experiment.

In Graph V, the average time per column for all *S*s has been plotted against the successive units of practice, or days, and the resulting trend shows that speed of performance increased in

both conditions, but 100% more in the 'tension' than in the 'normal' curve. The points on the curves represent an average of 100 measures, and hence give quite a reliable estimate. Moreover, there is a tendency for the 'normal' curve to round off toward the end, as though approaching an asymptote, whereas the 'tension' curve is steepest toward the end. Evidently, the advantageous effect of tension, instead of decreasing with practice in the function tested, grows more and more pronounced.

GRAPH V. PRACTICE CURVES OF TIME



UNITS OF PRACTICE (groups of 6 examples)

Accuracy presents a different picture. It gives results which are comparable with those for recall efficiency in the last experiment. Practice neither increased nor decreased the initial advantage in favor of tension. Evidently in addition, as in learning, it is the speed rather than the quality of the performance which is increasingly benefited by tension as practice renders it habitual. Any theory explaining the beneficial influence of muscular tension upon thought must take this fact into consideration.

TABLE IX

Influence of Practice on Accuracy: Average Number of Examples Correct per Day

NORMAL			TENSION			DIFFERENCE	
First Half	Second Half	Gain in Accuracy	First Half	Second Half	Gain in Accuracy	Greater Gain With Tension	% Greater
5.0	5.2	.2	5.3	5.5	.2	0	0

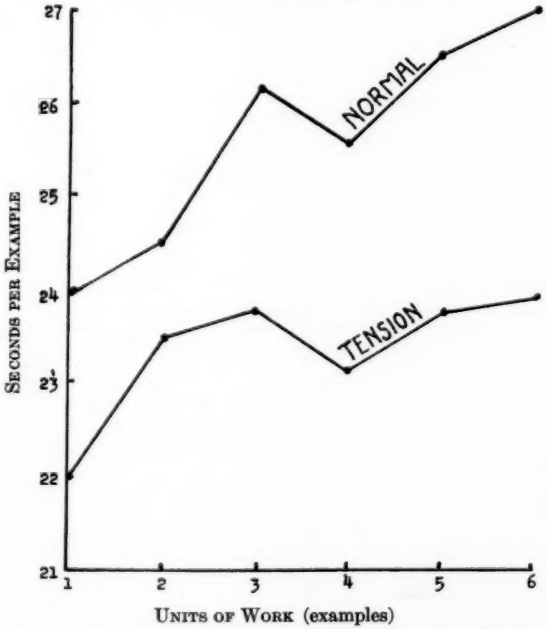
On the average, there is a slight increase in accuracy for both conditions with practice, but neither condition is more

benefited than the other. The accuracy curves for the two conditions run parallel to each other, as did the recall curves in Experiment II.

We may conclude, then, that practice increases the initial superiority of the tension condition, where speed is the criterion, but does not alter it where the criterion is accuracy.

(C) *Influence of fatigue.* The present experiment differs from the preceding in the fact that several measures were obtained

GRAPH VI. WORK CURVES OF TIME
Average number of seconds per example



at each sitting. This makes possible the plotting of daily work curves for each condition, showing the influence of tension on fatigue. In Graph VI, *time* per column has been plotted against successive units of work, or columns added, during an average work session. Two curves are thus secured, one for tension, and one for normal.

The points on the curves represent an average of 110 measures, since the curves are made by averaging 10 sessions

for each of 11 *Ss*. The very marked widening which occurs can therefore be relied upon as a true indication of the trend. Whereas the decrement under tension is only 2 sec., that under normal is 3 sec.; moreover, the tension curve shows a tendency to level off, while the other continues steep. The jog in the middle of both curves is accounted for by the fact that at this point the display cards were shifted, and the *Ss* got a fresh start.

As in the case of practice, when *accuracy* is the criterion, the results are different. To begin with, in the majority of cases, there was no accuracy decrement under either condition, but rather an increment perhaps due to warming up. Hence we can compare the two conditions only as to amount of increment, to see which shows the greater. This is done in Table X, where the two halves of the work session are compared.

TABLE X

Influence of Fatigue on Accuracy: Average Number of Examples Correctly Done

NORMAL			TENSION			DIFFERENCE	
First Half	Second Half	Gain in Accuracy	First Half	Second Half	Gain in Accuracy	Greater Gain With Tension	% Greater With
2.4	2.5	.1	2.6	2.7	.1	0	0

It is plain that in general there is no marked difference between the results for the two conditions. This agrees perfectly with the previous results for the accuracy criterion. Neither practice nor fatigue changes the ratio of efficiency between the two conditions, when quality of work is the characteristic measured.

We may conclude, therefore, that the effect of fatigue is to enhance the initial superiority of the tension condition, where speed is the criterion, but that with accuracy as a criterion the ratio is not altered.

Summary. The purpose of Experiment III was to determine the influence of tension on the speed and accuracy of adding columns of digits. The following facts were discovered. (1) Tension improves the absolute speed and accuracy of performance in adding columns of digits. (2) As a result of practice the initial difference of *speed* in favor of the tension condition is enhanced; but the *accuracy* ratio tends to remain constant throughout the practice period. (3) With the onset of fatigue the speed of performance suffers less decrement under tension, but the accuracy ratio tends to remain the same.

EXPERIMENT IV

The purpose of this experiment was to test further the findings of Experiment III with regard to the influence of tension on the decrement from continuous work, or *fatigue*. We wished to

determine what influence tension would have on the speed decrement under straight speed instructions, uncomplicated by accuracy, where practice effect had been eliminated. A type of mental work involving rapid perception was chosen, a modification of the color-naming test. The 10 *S*s used had already practiced the tests till improvement from practice was negligible.

Gummed letters, ¼ in. high, were mounted in rows, 10 rows of 10 letters each, on cardboard sheets 12 × 12 in., in haphazard order. Five types of cards were made, depending on whether they contained all possible arrangements of 2, 4, 8, 16, or 24 distinct letters. During a work session, *S*

TABLE XI

Absolute Ratio Between Tension and Normal Scores

S	Average Number of Seconds per Repetition of Card: Time					
	Normal	Tension	M_1-M_2	P.E. M_1-M_2	Difference times P.E.	% less Time under Tens.
Bl	24.6	25.5	−0.9	.28	3 ¼	−3.6
Ey	28.8	28.6	0.2	.213	1	0.7
Ko	30.4	28.9	1.5	.409	3 ¾	5.0
La	26.0	22.6	3.4	.252	13½	13.0
Mc	32.9	32.7	0.2	.267	¾	.06
Od	26.3	24.8	1.5	.33	4½	6.0
Os	27.9	26.7	1.2	.34	3½	4.3
Ri	21.7	19.5	2.2	.23	10	10.0
Ru	27.1	26.0	1.1	.334	3¼	4.0
Th	25.3	25.7	−0.4	.365	1	− 1.6
Av.	27.1	26.1	1.0	.134	7½	4.0

N = 1,000 100 measures for each subject.

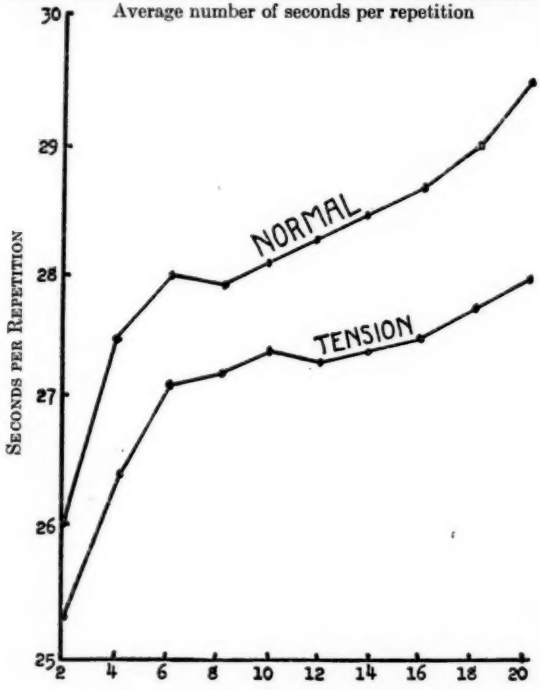
would read one card through at top speed 20 consecutive times. Time was recorded for every repetition of the card, thus giving 20 consecutive measures of speed from which to plot a work curve. The accuracy factor was eliminated from the results by having the *S*s correct mistakes as they read. Mistakes were very rare. Every one of the 10 *S*s was put through all 5 types of card, under both tension and normal conditions, or 10 work sessions. This gave 100 separate work curves in all.

Results of Experiment IV: (*A*) *Absolute ratio between tension and normal scores.* By finding the average time per repetition for the entire 100 measures secured from every *S* under each condition, we can secure quite a reliable estimate of the difference in efficiency of the two conditions. Table XI gives these, as well as the general averages for all *S*s combined. We find that for all *S*s the mean time per repetition under normal is 27.1 sec.; that under tension 26.1 sec. The difference is 1 sec., or about 4%. Though small, it is quite significant statistically, being 7.5 times its probable error. Perception, then, of the type involved in this task, is speeded up by the use of muscular tension.

(B) *Influence upon fatigue.* The important question is how tension affects the work decrement. Only one type of card gives

true decrement curves. With the others, fatigue is concealed by warming up and there is a slight increment. Hence we have divided the results into two parts, the first dealing with the results from the two-letter type of card, where a decrement appeared; the second combining the results of the other 4 types, where an increment appeared.

GRAPH VII. WORK CURVES OF TIME: TWO-LETTER COMBINATION



(1) The following table shows the average results for the two-letter combinations. There is 36% less decrement under tension.

TABLE XII

Influence of Fatigue on Speed: Seconds per Repetition of Card for Two-letter Combinations

NORMAL			TENSION			DIFFERENCE	
First Half	Second Half	Speed Loss	First Half	Second Half	Speed Loss	Smaller Loss With Tension	% Smaller
27.6	28.7	1.1	26.8	27.5	0.7	0.4	36

In Graph VII, on the preceding page, the average work curves for the two conditions are compared, resulting from the two-letter cards only. Considerable decrement appears in both, but that for the normal condition is much greater. Both show a very pronounced initial spurt, which continues only through the first 6 repetitions.

(2) Table XIII shows the average results for the 4, 8, 16, and 24-letter cards. The difference is more clear-cut than for the 2-letter cards. There is 100% greater increment with tension than with the normal condition. It is a safe assumption that tension is exerting a beneficial influence here in counteracting fatigue and allowing warming-up to appear.

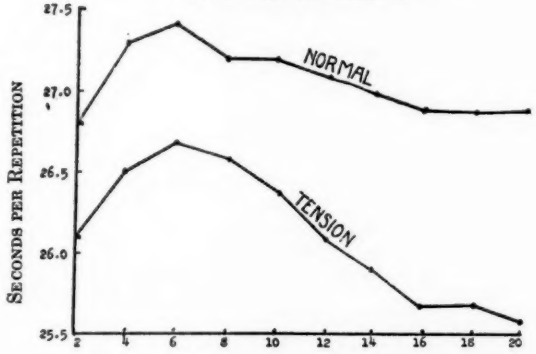
TABLE XIII

Influence of Fatigue on Speed: Seconds per Repetition of Card for Higher-Letter Combination

NORMAL			TENSION			DIFFERENCE	
First Half	Second Half	Speed Gain	First Half	Second Half	Speed Gain	Greater Gain With Tension	% Greater Gain
27.2	26.9	0.3	26.4	25.8	0.6	0.3	100

Graph VIII compares the normal and tension work curves. If the beginning and end are compared, the normal curve shows a slight decrement. The tension curve, on the other hand, shows a slight increment. If the initial spurt is disregarded, however, both show increment, but tension the greater amount by far. Each point on these curves represents a mean of 40 points.

GRAPH VIII. WORK CURVES OF TIME: AVERAGE OF ALL LETTER COMBINATIONS



Summary. The results of Experiment IV may be summarized as follows: In perceptive operations of the type here used, involving the continuous recitation of letters in scrambled order, not only does the tension condition give a higher absolute level of efficiency, but it is less susceptible to the effects of fatigue than the normal condition.

General Results and Conclusions

Table XIV is designed to bring together the results of all four experiments with regard to the absolute difference in level between performance under tension and under relaxation. The results are grouped under the three main criteria used, time, recall, and accuracy. The letter 'N' refers to the number of measures from which the average was computed. *Under all criteria of measurement, then, tension proves the superior condition.* The individual scores are in the main consistent. In Experiments II and III every *S*, without exception, gave better

Table XIV

Absolute Ratios Between Normal and Tension Scores

TIME
(Seconds per Unit)

	Normal	Tension	Less with Tension	P.E. of Diff.	% Less with Tension
EXPERIMENT I. (N = 90) Learning Nonsense Syllables	157	137	20	4.32	13
EXPERIMENT III. (N = 660) Adding Columns of Digits.	25.7	23.3	2.4	.32	10
EXPERIMENT IV. (N = 1000) Perception of Letters	27.1	26.1	1.0	.134	4

RECALL
(Items per List)

	Normal	Tension	More with Tension	P.E. of Diff.	%. More with Tension
EXPERIMENT I. (N = 90) Learning Nonsense Syllables	3.9	4.4	.5	.198	13
EXPERIMENT II. (N = 500) Learning Paired Associates	5.3	6.0	.7	.107	13

ACCURACY
(Examples correct per day)

	Normal	Tension	More with Tension	P.E. of Diff.	% More with Tension
EXPERIMENT III. (N = 110) Adding Columns of Digits	5.1	5.4	.3	.026	6

results under tension. In Experiment I, two-thirds did better, and in Experiment IV, 80% excelled under tension. Where individuals failed to conform to the general rule it was probably due to the conditions of the particular experiments, although it is possible that individuals differ in their susceptibility to improvement from increased tension.

Taking into consideration all the facts brought out by this research, the following general conclusions can be drawn.

(1) That muscular tension, of the form and amount used by us, does increase the efficiency of mental work of the kinds tried. The amount differs with individuals.

(2) That the increase in efficiency gained by tension is enhanced with *practice*, where speed is the criterion, but remains constant where other criteria or sets are used.

(3) That the added efficiency gained by using tension tends to increase as the subject grows more *fatigued*, when speed is the criterion, but remains constant where other criteria are used.

Theoretical Discussion

The question remains as to the manner in which increased muscular tension exerts its influence upon mental efficiency. We are justified only in outlining the possible explanations and suggesting what bearing our results have upon them. It is probable, but not certain, that the muscular tension normally accompanying intense thinking functions similarly to the tension used in this experiment.

The possible theories are grouped roughly under two heads, depending on whether the influence is thought to be exerted mainly through nutritive, or mainly through neural channels.

(*A*) *Through Nutritive Channels.* Increased activity stimulates various vital processes. Blood supply is altered, breathing changed, glands activated, etc., by muscular exertion. The effect, however, is not necessarily beneficial to thought. The exertion in this experiment accompanied the mental work; hence its effects must have been direct, no time being given for roundabout paths.

(*B*) *Through Neural Channels.* When the sensory-neuromuscular system is already active, it is thought that the threshold is thereby lowered to subsequent neural excitation and discharge. The tension may supply the previous activity. This again assumes antecedence in time. Theories supporting a simultaneous influence of tension on thought favor either an inhibiting or a facilitating effect.

The inhibitory theory seems to have been favored by Wm. James (1890) in his 'draughting off' hypothesis regarding motor concomitants; extra neural excitement is thereby withdrawn

from thought channels. Others suggest that it is irrelevant *motor* tendencies which are checked or inhibited by the static innervations accompanying strong attention. Irrelevant sensory stimuli might be excluded similarly. That the inhibitory theory is inadequate in the present study is shown by the fact that our subjects were not subjected to distracting stimuli from without, and that their irrelevant movements during the normal sittings were no more numerous than under tension.

Theories assuming facilitation are more numerous. The fact of a dynamogenic influence of simultaneous processes on one another has long been recognized. It probably accounts for the frequent cases where so-called distractors act as facilitators, and for all cases of summation of stimuli. The tensions used in this experiment may have functioned by setting up a flow of afferent impulses from proprioceptive organs in the muscles, better calculated to act as facilitators than the sudden, spasmodic distractors coming from without the individual.

Again, these tensions might exert a dynamogenic effect in the more indirect way suggested by Morgan, who found that external distractors acted as obstacles which, in the effort to overcome them, increased the speed and accuracy of mental operations, but did so at the expenditure of greater effort. Yet this should give more pronounced fatigue effects than the normal, and in the present study the decrement from fatigue was reduced.

If tension acted merely as a distractor, there is reason to suppose that its influence, like that of other continuous distractors, would gradually disappear by reason of adaptation.

The fact that reaction time in thinking, or speed, was most consistently affected by tension, seems to suggest that it was the muscular preparation involved which directly accelerated the responses. This effect would gradually increase as the tension became automatic through practice, which actually happened. This might explain why speed was influenced in a different manner from accuracy and recall efficiency.

It is probable that tension produced its effects in different ways at different stages in the process, or in several ways at once. Further experiment should be directed toward a qualitative analysis of the effects of muscular tension on thinking.

Because of the light which the study throws upon the rôle of muscular innervations in energizing and controlling thought, it paves the way for an objective treatment of the mechanism of 'will,' or of 'purposeful,' directed thinking. It offers an objective test of Washburn's hypothesis (1916) that "the motor innervations underlying the 'consciousness of effort' are not mere accompaniments of directed thought, but an essential part of the cause of directed thought."

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THE ACCURACY OF THE METHOD OF CONSTANT STIMULI

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Culler has recently published a series of articles which are very suggestive and give great promise for the future work of their author.[1] Some of his discussions, however, are marred by errors and mistakes which must not remain uncorrected. One of them is his determination of the probable error in the method of constant stimuli.

Culler proposes to find the probable error of both h and the limen directly from h itself. He conceives h as the measure of precision of the differential curve whose integral is fitted to the observed relative frequencies, and puts the probable error of h proportional to this quantity itself. This idea is clearly erroneous; h is derived from observations and its accuracy must be determined from the same data. This is done by comparing the observed results with those obtained by calculations. No formula which does not contain the sum of the squares of the deviations can be correct.

Culler, furthermore, misses the main point of my argument. He calculates the probable errors of h and c, but does not determine the probable error of the interval of uncertainty, as I do. My paper in the *Archiv* gives very detailed directions for finding the probable errors of both h and c.[2] My formulae are just an adaptation of the method of least squares to the problem of the method of constant stimuli. If you calculate h and c by least squares, you have no choice but to follow the same rules for the determination of the accuracy of the values found. No other procedure is logically consistent.

The constants of the psychometric functions are found with a fair degree of accuracy. Culler stops at the probable errors of these quantities and does not go any further. He forgets that h and c are not of immediate interest, but serve for the determination of the limen only. Here it is where the trouble lies. The limen is found by dividing c by h, and this process gives a very inexact result. The interval of uncertainty is the difference of the upper and the lower limen, and this subtraction further reduces the accuracy, roughly speaking, by one-third.

Calculating the interval of uncertainty by the $\Phi(\gamma)$-hypothesis is a roundabout process and the accuracy of the final result is

[1] Elmer Culler, Studies in psychometric theory, *Psychol. Mon.*, 35, 1926, 56-137. The article in question is: VI. The probable error of the limen and its derivation.

[2] F. M. Urban, Die psychophysischen Massmethoden als Grundlagen empirischer Messungen, *Arch. f. d. ges. Psychol.*, 15, 1909, 261-355; 16, 1910, 168-227.

very small indeed. This fact does not appear, if one considers the probable errors of h and c only without determining the resulting probable error of the interval of uncertainty. The errors of h and c combine in such a way as to produce a surprisingly large probable error in the interval of uncertainty. Table 68 of my paper in the *Archiv* gives the probable errors of the interval of uncertainty for the seven subjects of my lifted weights experiments.[3] The sums of the squares of the deviations are also given in this table, but the probable errors of h and c, being auxiliary quantities only, are not recorded. They are easily found in the way indicated in my article.[4] The probable errors of h and c in the example discussed by Thomson and Culler are 0.0021 and 0.64 respectively.

The decisive criterion for the reliability of any method is the probable error of the final result, in this case of the interval of uncertainty. The data of the table fully bear out my statement that calculation by the $\Phi(\gamma)$-hypothesis, *i.e.* the method of constant stimuli, is the least satisfactory of the four methods investigated. Observation or calculation by the method of just perceptible differences or calculation by Lagrange's formula give much more accurate results. Culler does not do justice to my argument, when he asks how the $\Phi(\gamma)$-hypothesis can be less exact than the arctan-hypothesis. I did not calculate the probable error of the interval of uncertainty by this latter hypothesis and, therefore, could not pronounce on the accuracy of this process. I merely showed that this hypothesis does not fit my observations as well as the $\Phi(\gamma)$-hypothesis and left it out of my further considerations.[5]

Culler then compares his results with those obtained by Thomson. This is a mistake if it is meant that these data are different from mine. The point of Thomson's criticism was settled five or six years ago. Thomson did not work out his suggestion; for, if it is consistently carried out, it leads back to the method of least squares and, therefore, to my own formulae. There is no real difference between his results and mine, provided that one does not stop halfway, as Thomson did.

My experience with this intricate problem of the accuracy of the method of constant stimuli is very curious. I started out with the idea that this method was the best of all. Its superiority in regard to experimental procedure is so marked that no experimenter can fail to be impressed by it. Is the same true in regard to the algebraic process involved, or is it at least as reliable as other processes? There exists just one way to decide this question: produce an experimental material which may be

[3] *Op. cit.*, 226. [4] *Ibid.*, 186 ff.

[5] I seize this opportunity to indicate a misprint in the last formula of my article (op. cit., 226). The second term in the bracket under the rootsign should read $b_1 S^2$.

worked out by the different methods and find the probable errors of the results. If you want to judge the accuracy of algebraic processes, you must apply them to the same material, for otherwise you are never sure that the better result may not be due to the superiority of the experimental material. This was done in my paper on psychophysical methods and my disappointment was great, when the results were so unfavorable to the method of constant stimuli.

At first I suspected a flaw in my argument or an error of computation, and did the work over and over again. Before proposing this result, which was so startling to me, I wanted to be sure that my argument contained no avoidable error. This is one of the reasons why my monograph on statistical methods in psychophysics,[6] which was brought out under the auspices of the University of Pennsylvania, does not contain the theory of the $\Phi(\gamma)$-hypothesis. Not being able to find the fault, I decided to publish my results such as they were, hoping that one of my reviewers would readily point out to me where the error is. In order to arouse contradiction I formulated the result rather more strongly than warranted by proclaiming the method of constant stimuli the least exact of all.

My hopes did not materialize for, with one exception, none of my reviewers thought it worth while to mention this point. The one who proved the exception, was not particularly competent to judge such delicate questions and contented himself by printing the conclusion and putting an exclamation sign in brackets at the end of the sentence. I felt inclined to let the reviewer know that I shared his sentiments and would welcome an answer. At that time I would have been very glad to be shown that my deduction contained an error, for my results confront us with the puzzling question, how it comes that the method of constant stimuli is so disappointingly inexact in its final result.

Things did not move for several years and I decided to look for help, since nobody offered voluntary assistance. At this time Thomson began the long line of his brilliant contributions to psychophysics and he looked to me like the providential man who might help me out of my quandary. So I wrote to him, explaining my difficulty and asking him to look into this matter. I mentioned my suspicion that the determination of the probable error in the method of constant stimuli contained an error. This letter, if I remember correctly, was written in the winter or spring of 1914.

This was a very good idea, although Thomson's criticism was, perhaps, undeservedly hard on my efforts, even if it had

[6] The application of statistical methods to the problems of psychophysics, 1908.

been entirely justified, which it was not. Thomson set himself to work and finished in a comparatively short time a very remarkable paper on this problem. Any reader of his article must be struck by the correctness and originality of his starting point, and by his obvious failure to arrive at a satisfactory result. His formulae are forbidding, and he evidently was frightened by them, for he devised all kinds of shortcuts and approximations to make them manageable at all. The outcome, of course, is that his final results are not much better than mere guesses.

That paper placed me in a difficult position. I clearly saw the merit of Thomson's idea. On the other hand, he had not shown me an error in my argument, but merely proposed another starting-point. It did not necessarily follow that I was wrong if Thomson was right. It also was plain to me that the numerical work would have to be arranged in an entirely different way, if his suggestion was to be taken seriously.

There were two alternatives before me. The first was to do the numerical work seriously, without shirking any computation no matter how laborious it might be. This work would be tedious and not very conclusive. Supposing that I obtained the same results by Thomson's formula and by my own, this coincidence would appear accidental.

The other alternative was to work Thomson's suggestion out to its logical end. My formulae are derived by least squares and are right, provided that no error crept into my deduction. The method of least squares contains no errors or contradictions, for the combined efforts of so many investigators would have brought them to light during the century which has elapsed since its invention. Supposing that Thomson's suggestion is right, it ought to be possible to deduce from it the well-known formulae for the probable error in the method of least squares. I saw that this work would be unusually hard; but I did not realise how difficult it would be, for otherwise I might not have ventured upon it.

Several lucky strikes helped me along and by 1920 I was in the possession of the complete solution, which is presented in my book *Die Grundlagen der Wahrscheinlichkeitsrechnung und der Theorie der Beobachtungsfehler*.[7] All the formulae for probable errors in least squares are worked out by Thomson's suggestion. His idea is right, but he did not work it out. His suggestion leads to my own formulae and his criticism is unjustified. Knowing the difficulties which are encountered on this way, I am the last to blame Thomson for not having gone to the end, and I am glad that my letter elicited such a splendid idea.

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[7] Pp. 223-238.

I communicated my results to Thomson, but not being fond of public controversy I did not answer him in print. This was a mistake. I had no right to assume that all workers in psychophysics would read my book on the calculus of probabilities. The book was primarily intended for mathematicians, so no reference was specifically made to psychophysical questions. It requires great insight and a very detailed acquaintance with the actual state of the problem to recognise that the deductions in my book are intended as a refutation of Thomson's criticism. One must not blame Culler for not knowing that my formulae and those of Thomson are essentially identical. I am sorry that my failure to answer publicly induced this gifted author to look for an error, where probably there is none and to use methods which are inadequate to this problem. My only excuse is that my answer would have come several years late, and after the war it looked futile to dig up old controversies.

The present situation, as I view it, is this. My presentation of the method of constant stimuli is in agreement with the method of least squares. This regards not only my determination of the probable error but also, as I may remark incidentally, the formula for the weight of the observation equations, with which Culler also finds fault. There exists just one way to refute my formulae: it must be shown that my deduction is in contradiction with one of the rules of least squares. None of my reviewers has done this. It is not sufficient to propose some new starting point and derive other fromulae; for if the new idea is in agreement with the method of least squares, it leads back to my own formulae, and if it is not, it must be thrown out of court at once.

At present I no longer hope that an error will be found in my determination of the probable error, which is rather a pity. It would be an easy matter to set the calculations right; but it is not easy to find the reason of the small accuracy of the method of constant stimuli. Neither is it easy to make up one's mind about the real standing of this method, which is from the point of view of the experimentalist, the best of all.

IS THE QUESTION OF ANALYSIS OF SOUND BY RESONANCE IN THE COCHLEA OR BY 'CENTRAL ANALYSIS' IN THE BRAIN STILL AN OPEN ONE?[1]

By G. WILKINSON, Sheffield, England

In a recent article in this JOURNAL Edwin G. Boring reviews the experimental evidence bearing on the theory of hearing.[2] He states his view that the question of the locus of tone-analysis is still open, but gives his vote for analysis by the brain according to frequency, rather than analysis in the cochlea according to pitch level through the mechanism of resonance. The present writer is a convinced adherent of the resonance theory, and to him it seems that the position taken by Boring is retrograde, abandoning, as it does, to the sea of doubt ground that has been laboriously reclaimed from the waves for physiology, and which is in process of being consolidated, with the prospect of its being rendered fertile. He is more concerned, therefore, to vindicate the resonance theory from doubts that have been cast upon it, than to criticise in detail the theory that has been put forward by Boring. To him it seems that Boring has omitted from his purview many weighty considerations in favor of the resonance theory, and that some of the arguments he advances as against that theory are open to question.

The novelty of Boring's thesis consists in his making perception of intensity the starting point. He sets out to formulate a comprehensive theory of intensity, leaving the other factors of sound perception to fit in as best they may. As he states this is a reversal of the usual procedure of starting from pitch perception, and then filling in the other factors—perception of intensity, direction, volume. There is this to be said for the latter method that the specific function of the auditory mechanism is to perceive sounds, *i.e.* tones, either simple or combined, whilst all the sense organs are concerned with the perception of intensity, volume, and (with the possible exception of smell) of localisation. It is natural in investigating a sense organ to pay special attention to the function which it alone fulfils. In the case of the ear, it may be supposed that the general plan of the organ, and the special mechanical features in which it differs from other sense organs will be correlated with the particular function of perceiving tones. Still, it is obvious, if we can discover the true method by which the organ performs one of its functions, it will in no way prevent, and may help, the explanation of its other functions.

[1]Manuscript received for publication, August 25, 1926.

[2]E. G. Boring, Auditory theory with special reference to intensity, volume, and localization, this JOURNAL, 37, 1926, 157-188.

Let us start then, as Boring has done, with perception of intensity. Boring's conception of the mechanism of the cochlea is that the disturbance due to the impact of waves of sound starts at the proximal or basal end of the basilar membrane, and that the extent to which the movement is transmitted along the membrane depends on the intensity of the impulse, the more intense disturbance travelling further towards the apical end than the less intense. His theory has a resemblance to the 'travelling bulge' theories of M. Meyer, Hurst, ter Kuile, and Watt, the difference being that he correlates the linear dimensions of the 'bulge' with intensity, and not with the duration of a single wave impulse, as do the latter theories. The older 'travelling bulge' theories gave a plausible solution of tone perception, but entirely failed to explain tone analysis. Similarly, Boring's theory might explain intensity perception, but could not explain intensity analysis. Boring states that "fusion of intensities is the most intimate kind of fusion known" but this is only true of intensities of the same pitch. Intensities of sounds of different pitch do not fuse to any considerable extent. Masking of sounds of one pitch by more intense sounds of another pitch occurs, but their fusion is not a recognised phenomenon. However many musical sounds we hear together we recognise that they have their relative intensities, as well as their relative pitches. The conductor of an orchestra immediately detects whether one or other of the instruments is playing too loudly, or not loud enough. The different sounds are perceived as separate entities each with its proper character (*i.e.* pitch distribution) and magnitude (*i.e.* intensity). This is difficult to explain except on the supposition of a discrete stimulation of the sense organ at different levels and with different amplitudes of movement at these levels.

The recognition of any difficulty in the interpretation of perception of intensity is only of recent origin, and is due to the formulation of the 'all or nothing' doctrine of nerve impulses. Before the work of Adrian and Keith Lucas appeared it was assumed that intensity was related in the simplest manner to degree of stimulation, or in other words depended on the amplitude of the displacement of the receptor organs. The 'all or nothing law' states that, if a stimulus to a receptor organ rises above a certain minimum (or limen), it will cause a discharge of energy to be propagated along the nerve fibre in connection with the receptor, and that the strength of the discharge is independent of any further intensity the stimulus may have beyond the minimum. The original experimental work on which the theory was based was concerned with motor nerves only. As there is no obvious difference in the structure, the chemical, and electrical reactions of motor and sensory nerves, there seemed no

reason why the same law should not hold good for sensory as well as for motor nerves. If this were so, how could the perception of intensity be accounted for? There appeared to be two possible explanations: either that for each increment of intensity an additional nerve fibre must be stimulated, or that more stimuli must be transmitted along the nerve fibres implicated in unit time. Boring adopts the first of these alternative views, and makes it the basis of the theory he has tentatively put forward. The second explanation was elaborated on theoretical grounds by Forbes and Gregg in 1916.[3] Recently Adrian has published experimental work on the current of action in sensory nerve fibres, which demonstrates conclusively that increase of intensity of the stimulus causes increase in the rate of rhythmic discharge of afferent nerve impulses.[4] "There is no evidence that an increase in the stimulus causes an increase in the size of the action current in single fibres, but the *frequency of the impulses* in the nerve trunk increases."[5] Thus we owe the extension of the 'all or nothing law' to sensory nerves to the original exponent of that law. This is a most notable advance, which will clear the ground of much unfounded speculation.

Adrian demonstrates that a constant stimulus applied *through the receptor organs* to a sensory nerve gives rise to a rhythmic discharge of afferent impulses, and that the frequency of the rhythm varies with the intensity of the stimulus. No doubt we may expect further communications working out these conclusions in greater detail, but the experimental results appear to be quite convincing. They even give us a hint as to the meaning of Fechner's logarithmic law of intensity of sensation. A study of the records reproduced in Adrian's paper shows an approximation of the number of nerve impulses to the log of the intensity of the stimulus. In the case of the current of action in the proprioceptor nerve fibres from the frog's gastrocnemius muscle, excited by stretching of the muscle, a weight of 5 grams gave a frequency of 120 impulses per sec., and a weight of 50 grams gave 310.[6] In another observation a weight of 10 grams gave approximately 180 per sec. and one of 100 grams gave approximately 360.[7]

An increase in the stimulus causes an increased frequency up to limiting value (corresponding no doubt to the absolute refractory period). The limit in the case of a frog's gastrocnemius-sciatic preparation was found to be 400 per sec. In the case of

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[3] A. Forbes and A. Gregg, Electrical studies in mammalian reflexes, *Amer. J. Physiol.*, 39, 1916, 234.

[4] E. D. Adrian, Impulses produced by sensory nerve endings, *J. Physiol.*, 61, 1926, 46-72. [Since Mr. Wilkinson wrote this article, there have been two other papers which further support the 'intensity-frequency' theory and the author's view: Adrian and Y Zotterman, *idem*, Pt. 2, Response of a single end organ, *ibid.*, 151-171; *idem*, Pt. 3, Impulses set up by touch and pressure, *ibid.*, 465-483.—Ed.]

[5] *Ibid.*, 72. [6] *Op. cit.*, 65. [7] *Op. cit.*, fig. 3, C and E.

the cutaneous nerves of a decerebrate cat at a temperature of 25°C the maximum number was 420, and in the afferent fibres of the vagus from the expanded lung, 450.

It seems to the writer that this 'intensity-frequency' law of Adrian's must dispose once and for all of the theory of 'central analysis' of pitch, or 'pitch-frequency' theory, as Boring calls it. Indeed, this theory had already worn sufficiently thin by reason of the recognition of the latent period of nerve stimuli before the recent work of Adrian shivered it to pieces. It could only be maintained on the basis of a number of arbitrary assumptions for which there was no particle of positive evidence, such as, for instance, that the refractory period in the auditory nerve might be much less (by some 60 times) than that of any of the nerves for which the period had been experimentally determined, and that frequencies up to 20,000 per sec. may not only pass the nerve unchanged, but also through the various cells and synapses that lie in the tract between the receptors and the cortex.[8] The maximum number of nerve impulses found in Adrian's experiments is 450. However, the question of the maximum number of impulses which can be transmitted by the auditory nerve has lost its significance so far as pitch perception is concerned, for if number of impulses means intensity it cannot at the same time mean pitch. On the 'frequency-intensity' basis, the number of impulses travelling to the cortex can bear no constant relation to the *number* of vibrations acting on the receptors. We are left, therefore, with relative position of the particular receptors on the basilar scale as the only possible criterion of pitch.

Incidentally, it may be remarked that the 'intensity-frequency' hypothesis offers a satisfactory explanation in the much more difficult case of visual perception. Such a problem as the appreciation of the intensity of light in an evenly illuminated field could not possibly be explained on the assumption that an additional nerve fibre is excited for each added degree of intensity of stimulus, but is readily explained by an increase in the number of nerve impulses transmitted.

What is the number of 'quanta' of intensity for a tone of single pitch? Intensity perception follows the Weber-Fechner law, but estimates of the magnitude of the steps vary somewhat widely. Knudsen[9] gives a 5% ratio; *i.e.* the least perceptable increase is 5% of the previous intensity. Fletcher[10] states that for intensities greater than 10^4 times that of the threshold value the ratio has a constant value of 10%. Fletcher and Wegel[11] give

[8]Boring, *op. cit.*, 181.

[9]V. O. Knudsen, *Phys. Rev.*, 19, 1922, 261. Cf. Fowler and Wegel, Audiometric methods, *Western Electric Co.'s Reprints*, 1922.

[10]Harvey Fletcher, Physical measurements of audition, *J. Franklin Instit.*, 196, 1923, 295.

[11]H. Fletcher and R. L. Wegel, Frequency sensitivity of normal ears, *Proc. Nat. Acad. Sci.*, 8, 1922, 5 f.

the maximum range of loudness of audible sounds (for tones from 512 to 2048 d. v.) as varying in different individuals from 1 to 10^6 to 1 to 10^7, *i.e.* the maximum intensity of sound over these regions of pitch is from one to 10 million times greater than minimum audibility. (Intensity in this case is measured by pressure change in the sound wave impinging on the tympanic membrane.) Maximum intensity is defined as the point at which sensation of sound merges into sensation of feeling. At intensities of stimulation slightly greater than the 'maximum' the sensation begins to be painful. Knudsen's basis of 5% steps, extending over a range of 1 to 10 millions gives us 663 quanta of intensity. The 10% ratio of Fletcher would give us 338 quanta for each distinguishable pitch within the range 512 to 2048 d.v. In any case the number is large. It may well be that future work on the rate of rhythm of impulse in sensory nerves will show that the number required is in excess of the 'quanta' which can be transmitted by a single nerve fibre. This, however, would create no real difficulty for the 'pitch-localisation' theory, as it cannot possibly be maintained that the movement of the basilar membrane giving rise to a sensation of a pure tone is confined to a single receptor. No exponent of the resonance theory (so far as is known to the writer) has assumed this exact localisation of resonant vibration; certainly not Helmholtz, whose celebrated experiments on 'shakes' had for their object the determination of the 'spread of resonance' along the basilar membrane in response to a pure tone. The loudness of the sound heard would presumably be determined by the sum of the 'quanta' transmitted by the nerves connected with the receptors involved, but a greater number of 'quanta' would be transmitted by the fibres connected with the receptors at the maximum point than by those connected with receptors further removed from the center of the disturbance, and our judgment of pitch would be determined by the position on the basilar scale of this maximum point. This is in accordance with Gray's 'principle of maximum stimulation'[12] which he derives from analogy with the sense of touch. As Boring has invoked this principle in support of his views on localisation,[13] he cannot deny its validity as applied to the determination of pitch locus.

What is the number of receptors in the cochlea? There seems to be considerable difference of opinion as to what constitutes a unit receptor in the cochlea. Some writers have reckoned the number of head plates of the arches of Corti; others have counted the number of nerve fibres going to the organ of Corti, as indicating the number of separate receptors. To the writer it seems that the unit receptor must be a hair cell. Retzius

[12]G. Wilkinson and A. A. Gray, *The mechanism of the cochlea*, 1924, 192.
[13]Boring, *op. cit.*, 168.

estimated the number of outer head plates of the pillars of Corti at 3848. There are, on an average, rather fewer than three hair cells to each outer head plate, say 10,000 in all. These are so arranged within the organ of Corti that those lying furthest outwards receive the greatest amplitude of vibration communicated by the fibres of the basilar membrane underlying them. The outer hair cells will be the ones in which the stimulus will first rise above the threshold value. Consequently, in an area vibrating in response to a pure tone, the 'quanta' will be clustered around the maximum point of disturbance, both by reason of the greater number of receptors stimulated at that point, and the greater number of 'quanta' transmitted by the individual nerve fibres connected with it. In the periphery of the vibrating area only the outer receptors will be stimulated, and the number of 'quanta' transmitted by them will diminish according to their distance from the center of the disturbance. This will serve to accentuate the 'maximum' point. If this line of reasoning is correct, it follows that the application of the 'all or nothing' doctrine to sense perception in the cochlea creates no embarrassment for the resonance theory, but rather gives it added support. Its position remains unshaken. The theory gives a reasonable explanation of all the facts of hearing, and is not in demonstrable disharmony with any of the known facts. The same cannot be said of any of the other theories which have been put forward.

But the positive evidence in its support must carry more weight than merely negative evidence, and to this positive evidence Boring has not, in the opinion of the writer, done full justice. It would exceed the due limits of this paper to analyse this evidence in detail, but the main points may be briefly summarised.

(1) The point of evidence which appeals most strongly to the writer is the fact of the regular graduation of the basilar fibres as to length, tension, and mass. The fibres increase in *length* progressively from the basal to the apical end of the cochlear scale. The spiral ligament, which attaches the fibres to the outer wall of the cochlear galleries, is graduated in the reverse direction, being very slender and delicate at the apical end, but dense and bulky at the basal end. The fibrils of which it is composed radiate fanwise from their attachment to the basilar fibres, and are disposed so as to give the impression that they are exercising tension on those fibres. Moreover, when cut along its length, the spiral ligament is seen to be composed of triangular leaves of tissue, corresponding in distribution to the individual basilar fibres to which they are attached. There can be little doubt that the spiral ligament is the organ of tension of the cochlea, and that the *tensions* on the basilar fibres are greatest at the basal and least at the apical end, and that they

are regularly and progressively graduated throughout the scale. Thus, the basilar fibres are differentiated by length and tension in the same sense so far as their tuning to respond to vibrations of different periods is concerned. Those that are shortest are tightest, and those that are longest are least stretched. The fluid which fills the cochlea acts as a 'load' on the basilar membrane, since, at whatever level the membrane is vibrating, the fluid between the round and oval windows and the vibrating sector must also be oscillating synchronously. Thus the fluid load is graduated proportionately to the distance of the vibrating sector from the round and oval windows. It is greatest on the apical sectors, and least on the basal, and diminishes progressively from the distal to the proximal end of the scale. The graduation of the sectors for mass is thus in the same sense as that for tension and length, so far as the differentiation of their periodicities of vibration is concerned. In fact, we have exactly the kind of differentiation we should look for in a set of strings tuned to vibrate to tones extending over a wide range of pitch.

(2) Yoshii's[14] classical experiment on the effect of prolonged overstimulation by tones of a single pitch show that the resulting damage to Corti's organ, and to the ganglion cells connected with it, is localised at levels of the cochlea which presumably correspond to the receptors of the pitches employed. This is a very strong argument for the 'pitch-level' as opposed to the 'pitch-frequency' theory. The criticism that has often been put forward that the resulting damage was more widespread than would be expected on the assumption of the validity of the resonance theory was met by Yoshii by the contention that the stimulus applied to cause the damage was essentially a forcing one, and also that the tones employed could not be regarded at pure tones of a single pitch.[15]

(3) The compliment of Yoshii's experimental investigations is to be found in the clinical investigations of Ritchie

[14]U. Yoshii, Experimentelle Untersuchungen über die Schädigung des Gehörorgans durch Schalleinwirkung, *Zsch. f. Ohrenhk.*, 58, 1909, 201-250.

[15]Boring states, "Yoshii himself concludes that the region affected is too broad for Helmholtz's theory and falls back on Ewald's" (*loc. cit.*, p. 184). I am unable to trace the reference on which this statement is based. Yoshii says, "The result of experimental investigation by continuous tones gives a further new strong support for the well known but hitherto not fully received resonance theory of Helmholtz. It should especially be noted that in each experiment the cochlea is damaged in one and indeed quite definite streak, corresponding to the height of the pipe used. By the experiment with pipe c^3 constantly the upper half of the basal spiral was damaged. With h^2 the effect was shifted a half to a whole winding higher, and with g 1½ turns higher. Consequently the higher the source of sound is the deeper lies the damage in the cochlear scale" (244). Yoshii criticises Ewald's acoustic camera adversely on the grounds, that the membrane in the camera does not conform to pattern, as its tension is not wholly transverse (245). He might have added that Ewald's membrane misrepresents the basilar membrane in every particular. It is not differentiated for width at different levels. Its tension is not regularly differentiated. The membrane is formed by painting a solution of rubber over the slit in the plate, and allowing it to dry. Such a membrane would certainly possess some tension given to it in the process of drying, but such tension would be fortuitous and irregular in distribution. It would not be regularly graduated as is the tension of the basilar membrane. Lastly, the 'oval' and 'round windows' of the camera are not disposed in the model with the same relation to the membrane as are those of the cochlea, and the loading by fluid columns would be entirely different.

Rodger[16] on occupation deafness. He found that the functional damage to the organ of hearing was confined to, or at all events affected earliest and most markedly, the range of tones predominating in the noises to which the patient was subjected in his daily work. Yoshii[17] quotes Putelli as having made similar observations.

(4) Recently a still further confirmation of the 'pitch-level' theory along the same lines has been brought forward by Mollie Weinberg and F. Allen.[18] They investigated the effects of fatigue due to stimulation of the ear by pure tones. They found that fatigue effect was maximum for tones whose pitch was in exact agreement with the fatiguing tone. When the difference between the fatiguing tone and the test tone is only 8 d. v. per sec. the fatigue effect is much less marked, and evanescent. When the ear is fatigued by two or more tones simultaneously, the results are suitably expressed by superposing two or more fatigue curves on the same graph.

It would appear as though the question of the existence of discrete pitch levels in the cochlea had been subjected to every possible test. Excessive monotonal stimulation in animals produces localised disorganisation of the cochlea at levels corresponding to the pitches employed (Yoshii). Less violent but more prolonged stimulation in man from occupational noises produces deafness for the tones corresponding in pitch to the noises to which the individual has been subjected (Rodger and Putelli). Overstimulation of shorter duration produces fatigue effects narrowly restricted to the tones employed (Weinberg and Allen). How could these results be explained otherwise than by the supposition that tones of one pitch affect certain receptors, whilst tones of another pitch affect other receptors? The experiments of Yoshii demonstrate further that these receptors are arranged in a progressively graduated series along the cochlear scale.

Of physical tests demonstrating the similarity in the behaviour of the ear to other forms of resonators we have a large number, all pointing to the same conclusion. Of these one may mention the experiments of H. Hartridge[19] as being specially ingenious and apposite. He employed a siren so arranged that an instantaneous change of phase of half a wave length in the successive pulses emitted by it could be produced. The effect of the change of phase was a short period of silence interrupting the flow of the tone. The deduction drawn is that the change of phase necessitated the damping out of the resonant vibra-

[16]Ritchie Rodger, Boiler-makers' deafness, *J. Laryng.*, 1923, 66.
[17]Yoshii, *loc. cit.*
[18]M. Weinberg and F. Allen, *Phil. Mag.*, Jan., 1924, 50-62, 126-141.
[19]H. Hartridge, A vindication of the resonance hypothesis of audition, *Brit. J. Psychol.*, 12, 1921, 142-146.

tions which had previously been set up within the cochlea, and the reëstablishment of a new series of vibrations.

The resonance theory alone offers a reasonable explanation of the main facts of audition. No other theory which has been put forward has any explanation to offer of the analysis of compound into simple tones, or the fact that alteration of the phase relationship of simultaneous tones, though modifying profoundly the form of the compound wave, produces no alteration in the quality of the sound heard. The occurrence of isolated pitch ranges of hearing in the congenitally deaf, and of isolated pitch ranges of loss of hearing (relative or absolute) in other cases of deafness, also point to the discontinuity of the receptors and of the associated nerve tracts concerned with the transmission of one pitch from those concerned with neighbouring pitches.

One may say that all the positive evidence bearing on the mechanism of hearing is in favour of the resonance theory. The only positive support which any alternative theory can claim is that furnished by Ewald's camera in favour of the 'pressure pattern' theory. The writer's contention is that this support is entirely fallacious, seeing that Ewald's apparatus entirely misrepresents the physical conditions present in the cochlea.

For the rest, it may be said that the alternative theories —the 'telephone' theory, the 'pressure pattern' theory, and the 'travelling bulge' theory — have had their origin merely in attempts to evade certain difficulties (real or imaginary) in the resonance interpretation. There is not a particle of positive evidence in favour of any of them. The mechanical possibility of the cochlea acting as a simple recorder of wave forms (as supposed by the 'telephone' theory) or being thrown into different patterns by simple tones of different pitch (as according to the 'pressure pattern' theory) or transmitting wave disturbances to varying distances (as in the 'travelling bulge' theory) has never been demonstrated. These theories are purely speculative. Whatever evidence we possess is against these possibilities. With regard to 'central analysis' of pitch frequencies recent physiological research is more and more strongly against the possibility of such a supposition.

On the other hand, possibly none of the positive evidence in favour of the resonance theory is so absolutely conclusive that exception to it cannot be taken, but the accumulation of evidence all pointing to the same conclusion, viz. that the cochlea discriminates pitches by localised vibration and that the localised vibrations are disposed in a regular progressive series, constitutes a very strong case for the resonance theory.

THE COURSE OF EXPERIENCE[1]

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The results of a previous study indicated that it might be possible to secure an adequate description of the initial moment of visual experience,[2] that the transition from one item to another in a visual stimulus-field had not been fully described, and that contour appeared to play an important rôle in the accrual of logical meaning. With these points in mind the present investigation was undertaken.

Technique. The technique is very similar to that of the earlier experiment, consequently a detailed description of the apparatus and procedure is here not necessary.[3] The following differences, though unimportant, must, however, be reported. A Whipple tachistoscope was used in place of one of the pendulum disk type. This, adjusted so the lever arm made a complete revolution at every release, was used for the exposure apparatus. The operation of this apparatus was noiseless and offered no distraction to the *Os*. The separation of the tachistoscope from the ground-glass exposure field was also slightly different. The tachistoscope was located 123 cm. behind the ground-glass field; the projecting end of the reflectoscope was 5 cm. to the rear of the tachistoscope. The *Os* were seated at a distance of 144 cm. in front of the field. The time of exposure was 60σ.

Observers. The *Os* were J. P. Guilford (G), assistant in psychology; Dr. Harry Helson (H), instructor in psychology; Max Meenes (M), Sage fellow in psychology; and Dr. Elizabeth F. Möller (Ml), honorary fellow in psychology. All were trained in the method of introspection.

Stimuli. Letters, numerals, and playing card forms (both red and black forms of every suit—red as well as black spades, and black as well as red hearts, etc.), were used as stimuli. These materials were spaced equally in rows of three on ruled paper exposure fields. The playing card forms were used as individual items. This uniform arrangement was at times varied. Occasionally a field with an irregular distribution of items was exposed; and now and then a blank field (*i.e.* the ruled paper background entirely devoid of forms) was exposed. These variations were introduced as check or control experiments.

I. THE INITIAL MOMENT OF EXPERIENCE

The first phase of the problem that we considered was the initial moment of experience. For this part of the work the following instructions were used.

Instructions 1. "A stimulus-field will be exposed on the ground-glass before you. Immediately following its presentation you are to describe your visual experience at the initial moment of the exposure. Fixate the 'spot' on the glass, holding fixation through the period of exposure. A 'ready' signal, followed by a 'now,' will be given prior to the exposure of the stimulus-field."

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[1]From the Psychological Laboratory of Cornell University. I am indebted to Professor E. B. Titchener for the opportunity of carrying out this investigation, and to Dr. L. B. Hoisington for his valuable suggestions.

[2]C. A. Dickinson, Experience and perception, this JOURNAL, 37, 1926, 330-344.

[3]For details of the method see the earlier study, *op. cit.*, 330-332.

Results. (*a*) *Experience as bidimensional.* The *O*s at first described their experience as bidimensional, *i.e.* as a mere spread. Examples are:

H, "The whole appeared as if a flash of light had passed across the visual field." "The whole thing spread at once."

G, "The center lighted up first and clearest, and the light spread out in all directions."

M, "At the initial moment the center became illuminated, then it spread all over the field."

Ml, "Around where I am fixating there is an extent of gray with a yellowish tinge to it, around this area another expanse of bright yellowish gray, and beyond that again another area less bright and less yellowish gray. There is an impression of movement, and I think the brilliant yellowish gray spreads out to the periphery."

(*b*) *Experience as tridimensional.* The *O*s later push back their descriptions of the initial moment of experience to tridimensionality. In place of 'spread,' a 'depth' experience came first. The light goes forward to the ground-glass before it spreads. Examples are:

H, "The welling up of the light doesn't take place so quickly that it cannot be experienced, but as it goes on all parts of the field come up simultaneously and give it arrangement."

G, "It seems as if a ball of soft, white light wells up at the center of the field of vision, drops a little to the left, and then spreads to the whole field. [*E*, "What do you mean by 'welling up'?"] I mean by 'welling up' that the experience seems to come forward a little and get larger."

M, "The flash began with low intensity at the center, at the same time coming toward me, and spreading very quickly in all directions." "That liveliness comes somehow from behind the ground-glass in the third dimension."

Ml, "The gray which is very yellow and bright comes from back of the field into it, and at the same time that it springs forward it spreads out." "Before I see anything else there is this yellow luminosity which springs forward into the field from somewhere behind it."

(*c*) *Experience as unlocalized filmy gray.* The final descriptions of the initial moment of visual experience had not even yet been reached. The *O*s pushed back their descriptions until tridimensional localization also became vague. Examples are:

G, "First I caught—and I don't often notice this—the impression that something is coming, something beginning—a sort of prelude. Then comes the stage I report so often, a burst of light which spreads and lights up the whole field." "I experienced the 'prelude' again. I think it is a vague grayish light experienced before the field appears."

M, "At the initial moment there are no definite boundaries." "A sudden illumination, the beginning of which I failed to get. Then it localized itself in three dimensions, it was bulky."

Ml, "There's something small and its gray, through I can't see any boundaries, and in the very seeing of it, it is gone." "There again was that 'something;' it is gray, but a gray that seems to be not on the field. It is almost transparent and hasn't any boundaries at all. The only thing that I am sure of is that it is gray and that it 'jumps' into experience." "The only experience that I have at first is of a peculiar kind of gray. I see this gray, and yet it appears to jump to the yellow. It is not a gray on the field, but something new, another knid of gray. That's all I can tell about it."

"I think there are two factors there, one a gray, the other a pressure; I can't tell whether it's a gray pressure or not, but it looks like it. The gray part of it is an indefinite filmy experience."

These reports clearly indicate that the initial moment of visual experience is unlocalized, but that it still carries reference to a visual quality.

(*d*) *Experience as 'spread' on the ground glass field.* We now return to the reports in which the *O*s described their experience as bidimensional, in order to illustrate the quality of the experience of the ground-glass, and the manner in which the 'spread' occurred. The following introspections illustrate these points.

M, "First experienced a dark gray field which hadn't yet lighted up. The gray seemed to grow, and then it suddenly burst into light."

Ml, "There is a dark gray filmy 'stuff' which rolls out toward the periphery and flashes." "There is a thick dark gray."

The words 'flash' and 'movement' were frequently employed to denote the 'spread' of the experience.

H, "Not sure whether the flash or the figures came first." "The flash showed a clear bright flare."

G, "Saw only one flash and it seemed much shorter than before." "I was sure I saw everything the first flash." "That time more of the field became clear all at once, but there was a movement of light, I cannot describe it, there seemed to be movement, yet the whole thing stands still."

M, "The flash itself came forward in the center." "Then at the same time the flash spread." "I don't know whether the spread is a movement or not, it does not seem to move from one place to another."

Ml, "What I seem to get is a gray flash and a yellow brilliancy." "The gray flashes, seems to move."

Most of the reports concerning the movement of the 'spread' are mere statements of meaning, but now and then, as the following introspections show, the *O*s were able to give a description of the process.[4]

G, "The light seems to shoot from these spots, a sort of fluctuating and shifting of the light."

M, "There were differences of brightness or intensity of illumination."

Ml, "The gray flash seems to shoot out from the center toward the periphery and to get less intense and thinner as it goes. I say 'goes' because I have this bright area at the center and then afterwards this less intense quality beyond it, which gives the impression of movement." "The point of fixation is dark gray, around that is a yellowish gray which is very bright. I say that this moves out, but what I see is that the periphery is a less intense yellowish gray. It gives the impression of movement. The experience is what you see when you wink your eyes, it means that the intense yellowish gray has moved out to the periphery." "The whole thing moves, it isn't a stationary pattern, the yellowish gray seems to quiver."

II. The Appearance of Form

The second aspect of the problem to be considered was the description of the appearance or emergence of form. The *O*s were instructed as follows.

[4] E. B. Titchener, Description *vs.* statement of meaning, this Journal, 23, 1912, 165-182.

Instructions 2. "A stimulus-field will be exposed on the ground-glass before you. In this field there will be forms. Immediately following the exposure you are to describe your visual experience relative to the manner in which these forms appear, giving special attention to the initial moment of their appearance. Fixate the 'spot' on the glass," etc., as in Instructions 1.

Results. (*a*) *Without instructions for form.* Under the instructions to describe the initial moment of experience (Instructions 1), the *O*s frequently, as the following introspections show, reported the emergence of form.

H, "The flash was experienced first and then I saw some black figures in a square." "First a yellow light, and then three rows of three objects."

G, "The first thing experienced was a field uneven in clearness and in brightness. Then forms which were very unclear, almost scrabbled—a sort of mottled field filled with gray spots like clouds." "The figures were very vague and indistinct. I just got the mere grouping and the fact that figures were present." "The field was not uniformly clear. *E*, "Describe the unclear."| The unclear appeared as though seen through a grayish cloud. Some figures or parts of figures seem to fuse with the background, their boundaries are indistinct. I mean by fusing that I can't tell where the background leaves off and the figures begin. Both contour and color are a sort of mixture of the two with little of no contour."

M, "First the bulky light, and then black indistinct forms; some light grayish splotches of low intensity, not forms at all, just opaque splotches. The center came first as black and then as form."

Ml, "By the time the gray flashed on there were some qualities present which later became figures." "I don't see any recognizable shapes or colors in the first instant. The clearest content is at the point of fixation and it is blacker than the rest of field, but even that isn't definitely outlined. It is just a visual quality and that's all. After that the forms then take definite shape."

(*b*) *With instructions for form.* In contrast to the foregoing reports compare the following given under the instructions (Instructions 2) for form.

H, "Experience seems to well-up into squareness all at once, comes at you, and then you begin to discern things within the whole."

G, "Forms were there immediately; the letters seemed to move upward to the right and then settle back. Only two of them were clear as definite forms."

M, "There were two forms of the playing card variety, but what ones they were I do not know. The rest of the field was just gray splotches."

Ml, "The field has on it things like shadows which are arranged all over. Suddenly they cleared up and showed definite form and color. At first they were just blurs."

(*c*) *The development of particular forms.* In the reports of 3 of the *O*s the emergence of form took on a temporal aspect, as the following examples indicate.

G, "The lower left was a sort of blackish red which became less black and more reddish, and then finally quite red."

M, "Near the fixation point I saw a dull red which later changed into a red heart."

Ml, "At first the whole outline of the figure was not taken in. I saw a little piece of the '9' which was just a gray quality and ther this grayness went away and I had the figure."

III. The Relation of Contour to Logical Meaning

In the third section of the experiment we were concerned with the relation of contour to the logical meaning of the forms. The following instructions were given to the *Os* in this part of the work.

Instructions 3. "A stimulus-field will be exposed on the ground-glass before you. In this field will be forms. You are to name the center form and to verify some other—the name and location of which will be called just in advance of the 'now' signal. At times a form other than the one called will be exposed. Immediately following the presentation of the stimulus-field you are to describe your visual experience from the time of the naming of the center form to the completion of the verification of the form called. Fixate the 'spot' on the glass," etc., as in Instructions 1.

Results. (*a*) *When one form is called and a different but similar form is exposed.* The tracing of form is at times quite regular, as the following reports show, when one form is called and a different but similar form is exposed.

G, (when the letter O was called and the letter D exposed), "The letter D flashed at the left, quite round at first, but recognized it as D as soon as it appeared; the two corners, however, were sharp. The experience was fleeting. I knew it as D although it looked like an O at first." (When the figure 8 was called and the letter S exposed), "The upper right became distinct and clear as a black S which grew quite large and tended to straighten out, *i.e.* the ends elongated."

M, (when Q called and G exposed), "I traced the form, that I first saw as round and O-ish, from the top to the left and down. When I started to go up to the right I was checked and then saw it as a G. I didn't see it at any time as a Q although that had been called out. The letter lost its O-ness as soon as I got to the gap with the cross bar."

Ml, (Y called and V exposed), "There seems to be a little gray darting quality which, as it travels, rolls up into itself so that it is always about the same size; it traces the letter which I expect. In this case it did not trace the whole letter but just the point where the two lines come together in a V, and a little beyond that to the right. The clearest part of the experience seems to be the part of the letter covered by this tracing. There in the background, was a more luminous and brilliant gray."

(*b*) *When one form is called and a different form is exposed.* The reports given below illustrate the course of experience when a form is exposed that is dissimilar to the one called out.

M, (M called and W exposed), "It appeared at first as a black form which was outlined which I could not make out except that it was black and had contours. Then the form changed, it expanded outward toward the top and inward toward the bottom; the center was merely a grayish spread. The center and the bottom were both very indistinct. Then the experience meant W. I was for some reason or another expecting a change in the opposite direction so this change seemed to be taking place in the face of opposition. It appeared as though a cloud-like experience had to be pushed away from it."

Ml, (K called and X exposed), "Where the two lines crossed there wasn't any mist or cloudiness. I traced the part of the K which would come at about that position on the X, and this tracing appeared as a little gray mist that formed an angle such as a K forms; instead of crossing it, it came back. This little gray thing was on top of the red, traced right over it, and appeared very clear. It almost stood out toward me just at that

point where the K is particularly different from the X. It was there for just an instant and then was gone."

Sometimes the *O*s failed to catch this tracing process, and instead reported a later stage in the change of one letter into another. The following are examples:

H, (Q called and G exposed), "Not sure whether it was G or Q. There is a quirk there like a Q, yet the Q is not complete. There is a break in the contour so that it looks like G." (V called and A exposed), "Uncertain about that A, it looked like an inverted V."

G, (3 called and 8 exposed), "A figure 8 appeared at the upper right, rather large, with only the upper part of it clear. I am sure, however, that it was an 8 because the upper half was a complete circle."

M, (Q called and G exposed), "Lower left experience not a Q because somehow there was a gap in the contour. I also noticed a bulging or increase of black in the experience just below the gap, and too high for the tail of the Q. It then meant G."

(*c*) *Form momentarily completed by projected imagery.* Sometimes, when the letter called is similar in contour to the one exposed, the gray process over-runs the boundaries of the actual form and completes the perception. Instances are:

M, (O called, C exposed), "That first appeared as a well defined black C which immediately sent out a grayish process over the gap and completed the O. Suddenly this disappeared, the letter was definitely a C again, and it remained so."

Ml, (E called, F exposed), "One seems to wait just a second for that extra stroke to come out on the E. During that time that area seems clear; then there's a little dark gray streak across where the stroke should be as if a ball of cloud were thrown along that line and the extra stroke made. That little speck of gray is clearer than all the rest of the letter, and the yellowish gray background upon which it appears it is more brilliant than the rest of the background. It doesn't form a perfect stroke and fill the letter out—it doesn't stay long enough for that—it merely indicates where that extra stroke should be. As it unrolls ahead it rolls up into itself behind, so there is just a small area visible at a time."

IV. The Transition from one Form to Another

The fourth phase of the problem that we considered was the description of the content of the periods of transition between the naming of one form and the naming of another. In this part of the experiment the following instructions were used.

Instructions 4. "A stimulus-field will be exposed on the ground-glass before you. In this field will be shown various forms. Immediately following the exposure of the stimulus-field you are to describe your visual experience in the period of transition from the naming of one form to the naming of another. Fixate the 'spot' on the glass," etc., as in Instructions 1.

Results. (*a*) *The emergence of the items.* The reports of the *O*s in this section bring out the fact clearly that there are no discrete additions in experience, that one item emerges from a preceding one, that it arises from a grayish mass into which, as it were, the preceding item had dissolved. Examples are:

G, "The letter F quite distinct, then expansion of the field in the direction of the upper right and O became clear. These shifts of clearness are

very much like the shifts one gets with eye-movement, as if I were now looking at one place, now at another, but I feel certain that my eyes were fixed on the spot throughout the experiment. The field swells or expands toward the right."

M, "The field widened diagonally, and as it widened I cognized the letter X—not, however, distinctly. It was like a 'pull' toward that letter. The letter became clearer and more prominent. [E, "What do you mean by a 'pull' toward the letter?"] The letter becomes larger, clearer, and attracted to one side of the field."

Ml, "The first thing in going from one letter to another is this black or dark gray strip which moves in the direction in which I am going. Next to this black there is a bit of gray film, and that's all I see for an instant; then the black moves over and draws the film after it, or moves over the film, and I see that the film is spread out and through it certain dark qualities appear. These qualities are not definite letters, but merely indications of forms. That is true of the letter I just left, and of the one to which I am going." "For just an instant there is nothing present but a dark streak, then behind it, attached to it, or existing under it there is a transparent murky grayness. The figure appears to be behind this grayness, yet I seem to see through it. The thing I call a black streak isn't definite like a rod or pole but shades into light gray. While the core moves that is all I see."

(*b*) *The development of color.* The reports below show that color also developes from gray, and that the regression from color is to gray.

Appearance of color:

G, "The letter C was first dark, then it became reddish."

M, "Saw a gray, then a red of medium saturation, then a saturated red heart."

Ml, "At first there are only gray shapes present. The shape near where I fixate is the clearest and nearly black." "The forms were spread out around the fixation-point. The one to the left became red almost immediately, but it was at first gray." "At first there was a darker gray which marked the position of the letter. This gray was somehow less dark than some of the others. The first I knew of color was that this was different from the gray of a black figure. It did not seem red until later." "Even the red letters are gray before they are red, but they are of a lighter gray than that of the black letters."

Disappearance of color:

G, "In disappearing the form seemed to shrink toward the center and at last was an unclear mass, more black than red—really a dark gray." "At the lower left was a red W which, as it went out, shrank into a dark grayish ball, quite dark at the center and fuzzy at the edges."

M, "At the same time the figure contracted and in one jump it lost its saturation and was gray." "At the left was a red pear-shaped surfacy form. In disappearing it moved away toward the left and lost its form and redness. In its place was a grayish form with just the faintest trace of red. Then the redness disappeared and it became a smoky splotch, scarcely differing from the ground-glass field."

Ml, "The top and the bottom of the figure lost contour and color just a little; then it was there, not as a red heart, but as a very light gray. It seems that the black figures go out by way of a dark gray, and that the red figures go out by way of a lighter gray."

(*c*) *Effect of irregular arrangement of the forms on the stimulus-field.* Occasionally a field was exposed in which the stimulus-forms were arranged irregularly. The effect of this is shown in the following reports.

H, "The figure at the center was something upside down. I got it as a spade after sensing 'upside-downness'."

G, "It looked like a J at the left. It caught my attention because it was so peculiar, *i.e.* upside down." "Very peculiar feeling when that appeared; very uneven and it did not hang together. Shortly after the first flash the meaning was changed, *i.e.* it meant some letters were missing."

M, "First got the meaning of heart, then of an inverted spade, and then of red." "The first thing I actually observed was the redness, then the upside-downness, then I saw that it was a club."

(*d*) *Effect of the blank experiments.* The blank or check experiments, in which the ruled paper background alone was exposed, gave the following results.

H, "A flash first and then nothing but ruled squares."

G, "First a grayish movement near the fixation-point, then the cross lines which moved outward in all directions. The sides of the squares seemed to bulge and become curved lines, and these seemed to extend out toward me as if the whole surface was convex, and then to sink back again with the shrinking of the surface."

M, "I don't know how to describe this experience. The fixation-point stood out, and the background behind lighted up. The illumination did not remain in one place; it actually moved, spread out from the center and shrank back again."

V. The Disappearance of Form

The final moment of experience in which the stimulus-forms disappeared was the fifth and last phase of the problem considered. The experiments of this section, however, were divided into two groups: (*a*) the general experiments in which the *O*s were free to describe the total visual field or any of the forms within it; and (b) the particularized experiments in which the *O*s were directed to describe some particular form whose position was called out just before the exposure was made.

(*a*) *General experiments.* The instructions for the general experiments were:

Instructions 5. "A stimulus-field will be exposed on the ground-glass before you. In this field will be shown forms. Immediately following the exposure you are to describe your visual experience at the moment in which the forms disappear. Fixate the 'spot' on the glass," etc., as in Instructions 1.

Results. Form disappears in a manner practically the reverse to that in which it appears as the following reports attest.

M, "The forms lost their articulateness gradually; they became more and more blurred, the contours became less and less distinct, the quality less and less saturated and bright. After the forms had lost their articulateness and saturation, they moved slightly and then disappeared."

Ml, "The field begins to contract and the forms lose outline. They are no longer forms but are diffuse, dull, opaque, cloudyish gray blurs. They moved a little toward the left and then disappeared."

(*b*) *Particularized experiments.* The instructions for the particularized experiments were:

Instructions 6. "A stimulus-field will be exposed on the ground-glass before you. In this field will be shown forms. Immediately following the exposure you are to describe your visual experience at the moment in which

some particular form—whose location will be given you—disappears. Fixate the 'spot' on the glass," etc., as in Instructions I.

Results. Samples of the reports obtained under these instructions are:

G, "The background around the F became gray and F disintegrated. The corners became round instead of sharp. The background was brightest at those points. Other parts became grayish and the boundaries roundish; saturation much decreased." "The left side seemed to grow upward and then the right side also. After that the figure seemed to disintegrate, to become indistinct and to disappear as an irregular mass."

M, "First lost distinct contour and saturation as it became gray. Then it contracted and thinned out until it was gone." "The black heart soon lost all contour and became less black, more and more dull, grayer, more diffuse, and indefinite in form. Finally disintegrated and vanished into the ground glass."

Ml, "The figure first lost a little of its blackness at the lower part, then at the upper, then its contour, and finally there was just a gray patch which was swallowed up by the darker gray of the background." "As it disappeared the black became nearer gray as if the mist had lightened it, and then the figure lost contour. The lower part of the 4 disappeared first and at one time it looked like a dark smoke ring in a lighter smoke. Then the gray mist which was getting darker and thicker swallowed up this circle."

Discussion of Results

The data we have presented carry through the course of visual experience from its initial moment to its termination. No attitudinal bias, either from the standpoint of process or from the standpoint of meaning, is set up by the instructions,[5] for the instructions mention neither process nor meaning, they simply call for description. In the early stages of the experiments, descriptions were limited to what appeared on the ground-glass field, but later the *O*s pushed their descriptions back beyond that. Either they were able to catch experience at an earlier stage and to retain it for description, or they developed the ability to ignore the stages which immediately followed the initial moment of experience and thus were able to give a more complete description of the initial moment itself.

We follow in our discussion the course of experience as it unfolded in the descriptions of the *O*s.

Relatively early in the experiments the *O*s report that the experience of form follows the appearance and the spread of the light which is bidimensional and which seems to "begin somewhere in the center and to throw itself over the whole field" (cf. I-a). Immediately following this "spread" the forms make their appearance (cf. II-a, b).

At a later stage in the experiment the *O*s report experience before the "spread." They push back their descriptions to a "kick of light," to a "welling up at the center of the field," to an

[5] E. B. Titchener, *A beginner's psychology*, 1915, 26 f.

experience which is tridimensional and which precedes the spread of light toward the periphery (cf. I-b). Such reports are given by all the *O*s, and some of them (H was unable to go back beyond this point) push their descriptions of the initial moment of experience back still further.

At this last stage the *O*s describe experience as an unlocalized filmy gray, as "something coming," "something beginning," which cannot be described. Something is experienced that is merely "a vague grayish light." This is not on the field, it precedes any distinctness of localization. It has at the first moment "no definite boundaries." Yet this something, indefinite and unlocalized as it is, carries a visual quality, which both directly and indirectly stated, is a filmy gray. Ml characterizes it specifically as a "pressury gray" which tends toward transparency and which appears to belong "more intimately" to herself "than out in space." The initial moment of visual experience according to the reports of our *O*s and under the conditions of our experiments is an unlocalized filmy visual experience light gray in quality (cf. I-c).

When we turn to the reports of experience as 'spread' on the ground-glass field we find that, as the spreading occurs, there is a "thick dark gray" which "moves out toward the periphery," which "rolls out," which "peels off," and leaves the luminous field exposed. This dark gray quality appears in many ways and at various times, but it is always recognizable by its opaque blue-grayness. After the field has become luminous "the brightness seems to shoot out" from various spots; there is "a fluctuating and a shifting of the light," often accompanied with the report that the "field itself is not uniform in brightness" (cf. I-d).

At this point we may note the use of the words 'flash' and 'movement' (cf. I-d). 'Flash' is used in a variety of ways and with a number of meanings. It may be "fairly large and circular," it may "continue all the time" of the exposure, it may be "gray" in quality, it may start with the 'spread,' or it may be localized behind the ground glass. 'Movement' is used in a similar manner. Few descriptions of these terms—as so frequently happens when categorical terms are used in report—were attempted; but from those that were given it seems fair to state that the words imply a rapid dimensional change in experience, *i.e.* a change of quality, density or texture.

Experience so far considered is devoid of meaning—other than the meaning of a filmy, unlocalized, visual quality. The meaning which later develops is an accrual, an overlay. The progression from this initial moment of experience to a particular meaning is by no means simple and direct; it is cut across and obscured in many ways. In any stimulus field we may find "eddies," "shifts," "quivers," "changes in intensity," and "changes in quality" before meaning is stabilized. When a highly specific meaning has accrued we may find that it has

been organized around a very meager core of sense data—as illustrated by the reports in which the *O*s characterize their experience by the words, 'flash' and 'movement.'

The emergence of form, reports of which were obtained under two sets of instructions (II-a, b),[6] further illustrates the complexity of the development of meaning. For H, the least practiced of our *O*s, form appears very quickly, nevertheless he too at times reported the 'flash' which all the other *O*s consistently reported as occurring before the appearance of form.

G describes the appearance of the forms in the 'free' reports (II-a) as "just the mere grouping," as being "quite unclear," like "gray spots," and as being so indefinite that he is unable "to tell where the background leaves off and the figures begin." In the 'constrained' reports (II-b) he describes the experience as "a grouping of figures, but these figures are very vague," and says, "the forms seem to rise up out of the background."

M, in the 'free' reports, characterizes the forms as "dark gray splotches," as "splotches of a different quality from that of the background." In the 'constrained' reports he characterizes the forms as "just opaque splotches," as "barely formed spots, just something there besides the illumination."

Ml, in the 'free' reports describes the forms which emerge in experience as being "like formless grayish shadows," "mere bits of grayish qualities." In the 'constrained' reports she describes them as being "like fragments of dark gray," and as "shadowy things which later become forms."

Forms are differentiated from the field first as a mere differences in quality. It is only later that contour is added. The initial appearance of form is nothing more or less than the appearance of a shadowy quality or a gray splotch. The thing most certain about the forms at this stage is that they have a grouping on the field. One of Dallenbach's *O*s, in an experiment undertaken to differentiate between attributive and cognitive clearness,[7] pushed back his description to an approximation of this indefinite beginning of form.

Following this indefinite stage, the form at the fixation point very quickly assumes a definite contour and takes on a particular meaning. The accrual of this specific or logical meaning is clearly illustrated in the experiments of Section III in which the development of particular forms is contrasted with the development of forms in general. In those experiments the *O*s were required to describe experience, in its various stages of development, until it stands forth as a fully named object.

The 'shadowy grayness,' which first appears, takes on a "loose, matlike texture," whose edges are "fuzzy" and barely discernible from the quality of the luminous field. Saturation has not yet accrued, nor has the solidifying effect of contour been added, at most the form is still "gray splotch." Saturation

[6] The bias of instructions is often credited with being productive of considerable warp in the results of an experiment—and this may be true—but in our case the 'free' (II-a) and the 'constrained' (II-b) reports differed but little if at all, as a comparison of the protocols clearly shows.

[7] K. M. Dallenbach, Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 3, 1920, 189 ff.

and contour, however, very quickly appear; saturation increases first, slightly in advance of contour, and it continues to increase after the contour has become relatively stable. After this there is a hardening up from 'splotch' to particular form. The loose texture solidifies, it becomes more surfacy and smooth; the color 'fades'[8] into its maximum; contour differentiates sharply; and the particular form stands forth as specific object. It apparently does not matter whether the object-color is red or black, for both of these qualities develop from gray, the red from a light gray, the black from a dark gray.

The tracing process, which was reported in the experiments of Section III, also plays an important rôle in the development of the logical meaning. The tracing is rapid, so rapid and fleeting that the *O*s sometimes fail to catch it and instead consider the accrual of logical meaning the result of factors which subsequently appear (III-b). Whether the contour be directly perceived as in the tracing process or indirectly perceived as in the observation of critical breaks and gaps, it is of paramount importance in the accrual of specific or logical meaning.

The experiments of Section IV clearly show that there are no discrete additions to experience, that the course of experience is characterized by gradual accretions. The letter called out sets up imagery which must be changed or accommodated, when a different letter is presented, to another contour. It is difficult for the *O*s to determine just what is taking place for very often nothing is, at certain periods in the change, clear or distinct. This is particularly true when the imagery aroused is wholly different in contour from that of the letter exposed. Distinctness of form resolves itself into a gray mass out of which arises another form that takes on a particular meaning.

For all the *O*s there appears to be a 'zero point' of meaning in the transition from one letter to another. This 'zero point' is visually a filmy grayness out of which arises the second letter. Here it is a question of establishing imagery to fit the sense data; in the previous section (III) it was a question of fitting the sense data to existing imagery. In both instances, however, there is a period of confusion or blur out of which steps the other letter.

In the transition from one letter to another, existing imagery must be dissipated and other imagery found to fit the sense data if the experience is to mean. In such cases, where experience is travelling in high gear, as it were, so many factors enter in that the actual tracing may be lost sight of and the filmy gray noted as a period of confusion. There is no break, however, in visual experience; the closest approximation to a break is the appearance of the visual gray.

[8]Analogous to the 'fade-in' of motion pictures.

The progressive overlay, which begins at the moment of localization and continues to the culmination of experience in logical meaning, is extremely complex. Even before the forms have begun to make their appearance a host of factors—the 'spread,' changes in brightness, dimensional shifts, and qualitative gradations—have run their course. These factors become no less complex in the emergence of form or in the development of particular forms. A great deal of what we experience visually may be considered analogous to the rise and fall of the ocean swell—there is, as it were, a rise and fall in the overlay, a progressive dimensional change, and with the shift in focality an occasional rise to logical meaning.

The overlay exhibits itself in a somewhat different manner in the actual moment of naming, particularly when a field is exposed with the stimulus-forms arranged in an unusual manner, *e.g.* placed irregularly or upside down. Items so arranged are likely to be named first.

In general it may be said that the experiential overlay manifests itself either as a progressive dimensional change which takes place gradually, or as a quick rise to logical meaning. Up to a certain moment the slower change in the pattern of the overlay is non-focal, it belongs to the total pattern and is part of the same unity. When it becomes focal and takes on logical meaning, the whole meaningful complex is still a progressive total for the meaning accrues with the shift and the emphasis in the focality of the pattern. In its main direction—its direction of progression—the meaning is perceptive, *i.e.* it means the quality that is there in experience. Yet it has the suggestion upon it of its place in the qualitative scale. Thus far it is comparative, but the specific concrete comparison comes only with the later shift in the total pattern, when the focus again dominates as at the beginning. At every shift in focality which culminates in logical meaning a new unity arises, not, however, by discrete additions, but by shifts inemphasis and in the direction of the focality of the meaningful complex.

Conclusions

From the results of our study we feel justified in concluding:

(1) That under our conditions the initial moment of visual experience is a filmy unlocalized semi-transparent gray.

(2) That visual experience is localized tridimensionally at first and that it later is localized bidimensionally, *i.e.* on the surface of the ground-glass field.

(3) That there is a continuous dimensional change with shifts in focality in this progressive overlay of experience.

(4) That meaning is a progressive overlay of experience, and that logical meaning is the climax of this overlay.

(5) That the accrual of contour is an important factor in raising this overlay of experience to logical meaning.

(6) That, in the progression to logical meaning, gray precedes color, and that, in the regression from this logical meaning, color recedes to gray.

(7) That, in the transition from the naming of one item in a stimulus-field to the naming of another, there is no discrete break in experience. One item disintegrates into a gray matrix out of which the following item arises as a new unity.

(8) That in the course of experience there is a gradual rise to a climax and then a gradual decay, and that the order of decay is the converse of the order of development.

(9) That, in the light of the data presented in this study, configuration as stated in *Gestalttheorie* has been emphasized beyond its legitimate bounds.

THE EFFECT OF ONE FORM OF REPORT UPON ANOTHER

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The purpose of this investigation is to determine the effect of one form of report upon another and totally different form of report. In witnessing an event or observing some objects, one might write out a narrative account of what he saw; or his report might take the form of answers to a series of questions based upon the event or objects seen. As is well known, the former is called narrative report, while the latter is known as interrogatory report. In this experiment, groups of subjects were asked to report under three different conditions; namely, simple narrative, simple interrogatory, and narrative plus interrogatory. With each condition an immediate recall and a delayed recall were taken. The delayed recall was taken after intervals of 30, 60, 90 and 120 days, depending upon the group.

The observational material used in this experiment was the Binet Object-Card, a rectangular sheet of orange-yellow cardboard, 33.5 × 40.5 cm. Attached to the sheet are two photographs, a red and white label, a white button, a one cent postage stamp and a penny.

Just prior to the observation of the object-card, the subjects were instructed that they were to be shown a cardboard with several objects attached, and that they were to observe it very closely, since immediately after observing the cardboard they would be asked to report what they had seen. The subjects came to the front of the room in groups of three to observe the material, and were given 30 sec. for inspection. After inspecting the cardboard they returned to their seats and made written reports of what they saw, in simple narrative form, simple interrogatory, or narrative plus interrogatory form, depending upon the group. The interrogatory consisted of 50 questions relating to the object-card.[1]

Twelve different groups of *Ss*, mainly sophomores in Washington University, participated in this experiment. Four groups reported in the form of a simple narrative account of what they had observed on the cardboard. Four other groups gave answers to the interrogatory; while another four wrote a narrative account which was followed by answers to the 50 questions. It will be understood that with the narrative, interrogatory, and narrative plus interrogatory, every group wrote an immediate recall and a delayed recall, the intervals of delay being 30, 60, 90, and 120 days. The number of *Ss* in each group was as follows:

	Form of Report		
Interval	Narrative	Inter.	Narr. plus Inter
30-day	39	37	49
60-day	46	41	54
90-day	31	35	26
120-day	26	42	24

At the expiration of the interval, the *Ss* were reminded of their previous observation of the cardboard, and were now asked to make the same kind of report as they had made immediately after that observation; *i.e.* nar-

[1]The questions were those suggested by G. M. Whipple, *Manual of mental and physical tests*, 1910, 298 f.

rative, interrogatory, or narrative plus interrogatory report, according to the group.

The narratives were scored by counting each item, right or wrong, as one. Thus a report, "an oval picture of a horse-drawn sleigh" would be given a credit of 4 points. Interpretations and indefinite statements were not counted. The interrogatories were scored by counting the answer to each question, right or wrong, as one.

There are three phases of the problem which may concern us in a discussion of the results. They are: (1) The effect of the immediate interrogatory upon the subsequent narrative after the time interval; (2) the possible effect of the immediate and subsequent narratives upon the subsequent interrogatory; and (3) a consideration of wrong items. We shall discuss these in the order mentioned. No statement can be made relative to the effect of the immediate narrative upon the immediate interrogatory in the double condition, as the conditions were not controlled to permit any such statement. Since different groups were involved for the various intervals, and their original learning scores were different, a cross comparison is not permissible.

TABLE I

Mean Number of Correct Items in Immediate and Delayed Narrative

Form of Report	Group	Interval			
		30-day	60-day	90-day	120-day
Narrative	Immediate	58.6	61.0	60.3	64.7
	Delayed	52.1	44.5	36.3	35.4
	Diff.	6.5	17.5	24.0	29.3
	Sig. Diff.	2.82	3.17	3.76	4.64
Narr. plus Interrog.	Immediate	65.5	57.3	56.7	50.1
	Delayed	62.0	44.3	48.5	39.4
	Diff.	3.5	13.0	8.2	10.7
	Sig. Diff.	2.8	2.9	3.58	3.6

(1) Table I presents the means of the immediate and delayed recalls at each interval, with their differences and sigmas, for the two conditions of simple narrative and narrative plus interrogatory. It would appear that the differences are thoroughly reliable except at the 30-day interval for both conditions and at the 90-day interval for the narrative plus interrogatory condition. The general tendency, however, appears to be consistent. Table I, it must be stated, gives simply the means of the raw scores. Since there were four different groups involved in each form of report for the four different intervals, it will be more convenient to express the amount of retention by stating the ratio, expressed in percentage, of the number of correct items in the delayed recall to the number of correct items in the immediate recall. Table II gives these percentages, and the same data are represented in graphic form in Fig. 1.

TABLE II

Percentage Ratio of Correct Items in Delayed Narrative to Correct Items in Immediate Narrative

Interval	Form of Report	
	Narrative	Narr. plus Inter.
Immediate	100	100
30-day	88.8	94.6
60-day	73.0	77.3
90-day	60.1	85.4
120-day	54.7	78.7

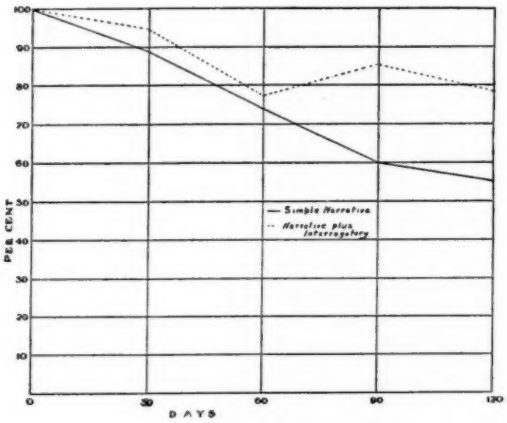

FIGURE 1

For the simple narrative condition, it will be observed that the curve of retention falls very nearly in a straight line from immediate recall to 90 days, and more slowly from 90 to 120 days, indicating a slight negative acceleration.[2] The narrative plus interrogatory curve is above the simple narrative curve at all four points. It is interesting to observe that, while the curve for the double condition runs at a higher level than that of the simple narrative condition throughout, the shape of the two curves is very similar up to the 60 days, after which the curve for the double condition shows a marked negative acceleration. It seems reasonable to conclude that the interrogatory report which followed the immediate narrative served to suggest items observed, and therefore contributed to facilitate spontaneous recall at subsequent intervals. This transfer effect, if we may call it thus, appears to be accentuated at intervals of 90 and 120

[2]A complete discussion of the retention curves under the simple conditions appears in an article by us on The recall of observed material, *J. Educ. Psychol.*, 17, 1926, 419 ff.

days, although the facilitating effect is consistent for all intervals measured, as suggested by a comparison of the two curves. The difficulty of revival would presumably be greater after the longer intervals, and there would be a greater opportunity for the operation of such associative cues as the interrogatory should furnish. Hence we might expect a greater effect after longer than after shorter intervals. This would be clearer, however, if the interrogatory immediately preceded the subsequent recall.

(2) In the second phase of the problem we are to consider the effect of the immediate and subsequent narratives upon the subsequent interrogatory. Here we are to compare the simple interrogatory condition with an interrogatory that was preceded by a narrative form of report. The means of the gross

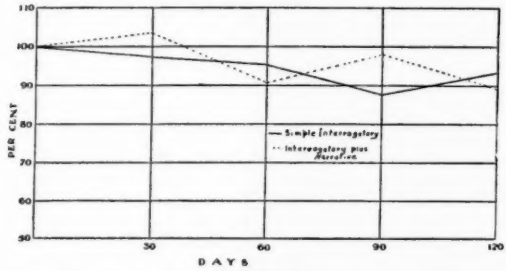

FIGURE 2

scores, immediate and delayed, with their differences and sigmas, are presented in Table III. The amount of forgetting as measured in terms of interrogatory report is relatively slight, and the differences are statistically unreliable. Again, since a different group was involved for each interval, it is better to show what percentage the number of items right in the delayed interrogatory constitutes of the number of items right in the immediate interrogatory; as in the conditions previously discussed. Such data, for the two conditions, are shown in Table IV and presented graphically in Fig. 2.

TABLE III

Mean Number of Correct Items in Immediate and Delayed Interrogatory

Form of Report	Group	Interval			
		30-day	60-day	90-day	120-day
Simple Interrogatory	Immediate	36.9	38.2	37.69	38.0
	Delayed	36.0	36.5	33.12	35.5
	Diff.	.9	1.7	4.58	2.5
	Sig. Dif.	2.0	1.72	1.63	1.5
Inter. plus Narrative	Immediate	39.2	38.6	34.3	36.2
	Delayed	40.7	35.1	33.9	32.4
	Diff.	1.5	3.5	.4	3.8
	Sig. Dif.	1.07	1.13	2.59	2.54

TABLE IV

Percentage Ratio of Correct Items in Delayed Interrogatory to Correct Items in Immediate Interrogatory

Interval	Form of Report	
	Interrogatory	Inter. plus Narr.
Immediate	100.0	100.0
30 days	97.7	103.8
60 days	95.5	90.7
90 days	87.8	98.4
120 days	93.4	89.5

The curves intercept at three different points, which indicates that no consistent tendency is manifested. We would conclude that the interpolation of the narrative report does not facilitate revival measured in terms of the interrogatory.

(3) Thus far we have said nothing about errors. In each recall for the various conditions there were a few errors; these errors, however, were highly variable and the differences indicate low reliability. We are not justified in drawing any conclusions regarding error scores, and for this reason tables showing error scores are omitted.[3]

CONCLUSIONS

(1) The interpolation of an interrogatory form of report immediately following a narrative report has a facilitating effect upon subsequent narrative recall at 30, 60, 90, and 120 day intervals. This effect is more accentuated at 90 and 120 days than at 30 and 60 days.

(2) Narrative report interpolated prior to interrogatory recall, however, does not facilitate recall or report measured in terms of answers to a series of questions.

(3) With respect to errors, no effects of one form of report upon another are observable for any condition measured.

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[3] Cf. K. M. Dallenbach, The relation of memory error to time interval, *Psychol. Rev.*, 20, 1913, 323-337.

THE EFFECT OF SERIAL POSITION UPON RECALL

By JOHN G. JENKINS and KARL M. DALLENBACH, Cornell University

The results of investigations undertaken to determine the effect of serial position upon memory and reproduction have been generalized, by many writers of elementary textbooks, into the 'laws' of primacy and recency. These generalizations have been justified on the ground that all the experimental results, from those of Bigham in 1894 to those of Warden in 1924, show that the first and last members of a series are best retained, and that the middle members are in positions of disadvantage.[1]

Recently, both the experimental results regarding 'primacy,' and the generalization based upon them have been challenged. Welch and Burnett, in 1924, obtained results (in a study that was undertaken specifically to determine the effect of primacy) that are diametrically opposed to the earlier findings.[2] They discovered that when the *S*s' tendency to 'think backward' during the exposure of a series is prevented, *i.e.* when the *S*s have no opportunity of rehearsal or review, "the alleged effect of primacy disappeared."[3] Primacy, according to their results, is effective only in so far as it makes for frequency. These unusual (one might almost say unorthodox) results have revived interest in the problem of primacy, and two investigations regarding it have been reported within the past few months.

In the first, that by Robinson and Brown,[4] results are reported which reaffirm the old findings. The authors worked over the data of two experiments on memory that had previously been performed in the Chicago laboratory, and discovered that the "advantages of both primacy and finality were pronounced," and that "primacy shows even a greater advantage than finality."[5] They criticise Welch and Burnett's conclusions on the ground that the data were not correctly interpreted. In the light of the work that has already been done on the problem they feel that these results can best be explained, since Welch and Burnett's *S*s gave their reports immediately after the exposures, "in terms of the greater time interval between memorizing and recall in the case of the first syllables"[6]—an explanation which places Welch and Burnett's investigation outside the realm of the problem in question. The net result of Robinson and Brown's investigation is to return us to where we were before the publication of Welch and Burnett's study.

In the second and most recent investigation of the influence of primacy, that by Thorndike,[7] the results of Welch and Burnett are confirmed: "primacy in and of itself has zero potency."[8]

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[1] John Bigham, Memory, *Psychol. Rev.*, 1, 1894, 457.
M. W. Calkins, Association: An essay analytic and experimental, *Psychol. Monog.*, 1, 1896, No. 2; Association, *Psychol. Rev.*, 3, 1896, 39, 44 f., 48.
W. G. Smith, The place of repetition in memory, *Psychol. Rev.*, 3, 1896, 21-31.
H. Ebbinghaus, *Grundzüge der Psychologie*, I, 1902, 624-626.
A. Pohlmann, *Experimentelle Beiträge zur Lehre vom Gedächtnis*, 1906, 92-138.
W. A. Owens, unpublished, reviewed by E. S. Robinson and M. A. Brown, this JOURNAL, 37, 1926, 539 f.
E. O. Finkenbinder, The curve of forgetting, this JOURNAL, 24, 1913, 8-32.
G. J. Rich, Directed attention and learning, *J. Educ. Psychol.*, 8, 1917, 239-240.
R. S. Woodworth and A. T. Poffenberger, *Textbook of experimental psychology*, 1920, 71 f.
C. J. Warden, Primacy and recency as factors in cul-de-sac elimination in a stylus maze, *J. Exper. Psychol.*, 7, 1924, 98-116.

[2] G. B. Welch and C. T. Burnett, Is primacy a factor in association formation, this JOURNAL, 35, 1924, 396-401.

[3] *Op. cit.*, 401.

[4] E. S. Robinson and M. A. Brown, Effect of serial position upon memorization, this JOURNAL, 37, 1926, 538-552.

[5] *Op. cit.*, 552.

[6] *Op. cit.*, 543.

[7] E. L. Thorndike, The influence of primacy, *J. Exper. Psychol.*, 10, 1927, 18-29.

[8] *Op. cit.*, 18.

Regarding the accuracy of Thorndike's experiments there can be no question, but serious objection can be raised to their relevancy, and to the conclusion that he draws from them. Thorndike's *Ss*, in the first experiment, for example, estimated the lengths of 25 paper strips which varied in length by ¼ in. steps from 5 in. to 11 in. The series were presented 10 times, thus giving 10 estimates for every length from every *S*. Because the first estimates are not repeated more often in the last 7 of the 10 responses than the second or third, Thorndike concludes that primacy in and of itself has no potency. The experiments were carefully planned and executed, and the results most clearly establish the fact that the first estimate is not indicative of the later ones. So far Thorndike has established his thesis. But what have these results to do with memory and association? The *Ss* were directed to estimate, not to remember their estimates. Unless an association was formed between the first estimate of a given length and that length, how could the efficacy of primacy be tested? And what guarantee have we that such an association was formed? Mere contiguity is no guarantee. If no associations were formed—and Thorndike's results clearly indicate that they were not—then the experiments are irrelevant to the problem in question and take their place with the experiments on judgment. We might just as well look to judgments in the lifted-weight experiment for data upon this problem as expect Thorndike's experiments to contribute to its solution.

The effectiveness of primacy in serial learning is not questioned by Thorndike, but the doctrine of primacy which has grown up around it is rejected as fabricated. "The experiments of Calkins . . . . ," he says, "do not, as we interpret them, necessarily show that 'the earlier one element enters into association with another, the more likely it is to be recalled with that than with any other with which it has been associated at a later period;' but rather that being first in a series of twelve paired-association tasks has an advantage over being in the middle of such a series. It seems probable that the advantage is not due to primacy in and of itself, but to the factors which make both the beginning and the end of a series better remembered than the middle."[9]

The potency ascribed to primacy, Thorndike maintains, can be explained by two corollaries of his general law of learning. (1) "That, other things being equal, the stronger a connection is, the oftener, and so the earlier, it will show itself. Being first does not make a connection stronger, but being strong makes a connection likely to be first." (2) "That when responses are connected with situations to which there are no pre-existing connections of more than infinitesimal strength, the first experience raises the frequency from approximately 0 to 1, the second experience of the same connection raises it from 1 to 2, and so on. The *relative* strength of the connection with its competing infinitesimals is raised to 1-to-0 by the first experience, and only to 2-to-0 by the second experience. This is an effect of frequency caused by position and is allied to the facts of diminishing returns and overlearning. It is different from the alleged primacy effect. If, in such a case, we have two different responses, one in first position and the other in second position of recurrence, each will be raised to a relative strength of 1-to-0 compared with the infinitesimals; and the one in first place will be at no advantage over the one in second place."[10]

Without holding a brief for the law of primacy, we fail to see how these corollaries explain all the experimental findings. According to the first corollary the first member of a series is reported most frequently, and reported most frequently first, not because it is the first member encountered, but because it is the most strongly associated. But why is it the most strongly associated? If we read the second corollary correctly,

[9] *Op. cit.*, 28.
[10] Thorndike, *op. cit.*, 28.

Thorndike holds that it is the most strongly connected because of the effect of frequency—a conclusion which corroborates Welch and Burnett's results.[11] But the effect of primacy, as Robinson and Brown[12] pointed out in answer to Welch and Burnett, appears even in those experiments that were specially designed to equalize the effect of frequency.[13] If the experiments were successful in this design, then the answer to the question must be sought elsewhere. Frequency will not explain primacy.

It was the hope of obtaining results significant for this problem that we worked over the data which we had previously gathered in our study on forgetting from the point of view of the effect of serial position.[14]

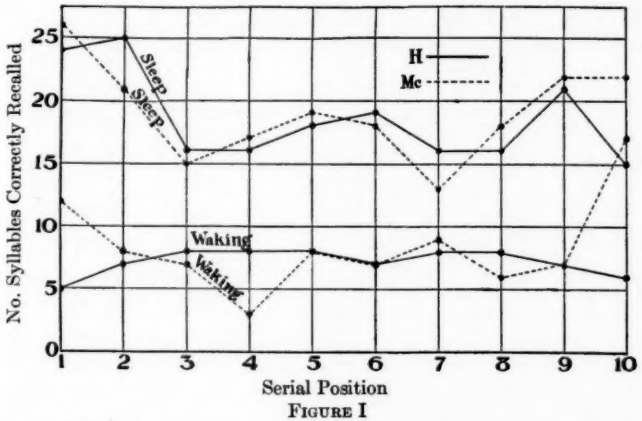

FIGURE I

Procedure. As the procedure is fully reported in the earlier study, to which the interested reader is referred for details, only the most important points will be mentioned here.

A series of 10 nonsense syllables was presented visually to two *O*s (H and Mc) until learning had come to the first correct recitation. The *O*s read the syllables aloud. Presentation was so rapid—at the rate of 0.7 sec.—that a review or rehearsal during learning was practically impossible. After learning the *O*s were instructed not to think of the series until they were asked to give a recall. After intervals of 1, 2, 4, and 8 hrs. of sleep or waking a free reproduction was called for. Except for 3 of the waking intervals, 8 experiments were completed at every one of these periods. Recall was always made in the experimental room with *O* seated at the apparatus in the position held during learning. *E*, the junior author, copied the reports as they were given orally by *O*.

Results. The total number of syllables correctly recalled by each *O* at every position within the stimulus-series and at every time interval during sleep and waking is shown in Table I. The total results, for each *O* and for sleep and waking, are also shown graphically in Fig. 1.

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[11] *Op. cit.*, 401. [12] *Op. cit.*, 543.
[13] W. G. Smith, *op. cit.*, 21-31.
[14] J. G. Jenkins and K. M. Dallenbach, Obliviscence during sleep and waking, this JOURNAL, 35, 1924, 605-612.

TABLE I

Number of Syllables Correctly Reproduced at Every Time Interval after Sleep and Waking and their Serial Position within the Stimulus-Series

O	Recall after	Interval	Serial Position of Syllables Recalled									
			1	2	3	4	5	6	7	8	9	10
H	Waking	1 hr.	2	4	4	5	3	2	2	3	5	5
		2 hr.*	2	1	1	2	2	2	3	3	1	0
		4 hr.†	1	1	2	1	3	3	2	2	1	1
		8 hr.	0	1	1	0	0	0	1	0	0	0
		Total	5	7	8	8	8	7	8	8	7	6
	Sleep	1 hr.	6	6	6	4	7	6	5	5	8	4
		2 hr.	6	5	3	5	5	4	4	3	4	4
		4 hr.	6	7	4	2	4	3	5	4	4	3
		8 hr.	6	7	3	5	2	6	2	4	5	4
		Total	24	25	16	16	18	19	16	16	21	15
Mc	Waking	1 hr.*	6	3	2	2	3	3	2	1	2	5
		2 hr.	2	3	2	1	2	2	6	3	3	3
		4 hr.	3	1	2	0	2	1	0	1	1	6
		8 hr.	1	1	1	0	1	1	1	1	1	3
		Total	12	8	7	3	8	7	9	6	7	17
	Sleep	1 hr.	7	5	5	5	4	5	7	6	5	7
		2 hr.	5	6	3	3	6	3	3	4	7	3
		4 hr.	7	6	5	5	4	5	1	4	4	5
		8 hr.	7	4	2	4	5	5	2	4	6	7
		Total	26	21	15	17	19	18	13	18	22	22

*Results of 6 series. †Results of 7 series.

In computing the results partial successes were not counted; only those syllables correctly reproduced were considered. The associative fusions and partial successes were disregarded; such cases however, were so few in number,[15] that had they been counted the results would not have been materially affected.

The effect of serial position, as these data show, is different for the two *O*s. Mc gives results that we have learned to expect: the syllables which occupy the first and last positions are recalled more frequently than those in intermediate positions. This holds true for the recalls after intervals of both sleep and waking, but the relative importance of primacy and finality is different under the two conditions. During waking hours syllables in the final position are reported more frequently than those in the first position, whereas after intervals of sleep the opposite relation holds true. From Mc's results alone we should have to conclude that primacy and recency are effective in the recall, and that the relative importance of these two positions varies with sleep and waking.

H's results, however, are so very different that they negate whatever conclusions that we might at this point draw from Mc's results. H recalls, after intervals of waking, fewer syllables from the first and last positions within the stimulus-series than from any of the intermediate positions. The curve is highest in the middle and falls at the two ends. Although the absolute difference is small and the data are few in number, the results,

[15] *Op. cit.*, 608.

as they stand, corroborate the findings of Welch and Burnett. After intervals of sleep, on the other hand, H shows a marked tendency in favor of the first part of the series. The curve of recalls is highest at the beginning and lowest at the end. If H's results were considered by themselves we should have to conclude that serial position was not effective after intervals of waking, and that primacy alone was effective after intervals of sleep.

When the results of the two *O*s are considered together we are forced to Thorndike's conclusion that primacy (and also recency) is not in and of itself effective, and that whatever advantage it has it owes to other factors.

According to the first corollary of his general law of learning that Thorndike proposes, the specific factor which gives primacy its advantage is the greater strength of that associative connection. The question then resolves itself into determining what factor or factors gives this greater strength to the connections occupying the initial position within a series; and, specifically, what factors were present in the experiments with Mc,

TABLE II

Number of Syllables Correctly Reproduced after Sleep and Waking at Every Position within the Stimulus-Series and within the Recall-Series

O	Position within series	Recall after Waking, order reported: first	second	middle	next last	last	Recall after Sleep, order reported: first	second	middle	next last	last
H	1	3	1	1	0	0	12	0	5	3	4
	2	4	0	1	2	0	2	6	6	4	7
	3	4	2	0	2	0	5	3	3	3	2
	4	3	2	0	0	3	0	2	12	0	2
	5	3	1	0	2	2	2	4	7	1	4
	6	2	0	1	0	4	2	1	7	4	5
	7	1	0	5	0	2	1	3	6	5	1
	8	2	1	0	1	4	1	3	6	4	2
	9	1	1	2	1	2	5	2	6	3	5
	10	1	1	0	1	3	2	4	7	2	0
Mc	1	12	0	0	0	0	24	0	1	0	1
	2	3	1	2	0	2	2	16	0	2	1
	3	2	1	1	1	2	1	3	9	1	1
	4	1	1	0	0	1	0	2	10	1	4
	5	1	1	1	1	4	1	1	13	1	3
	6	2	1	1	2	1	0	8	6	2	2
	7	0	1	2	2	4	1	1	8	1	2
	8	2	0	2	0	2	2	0	7	6	3
	9	2	0	1	2	2	0	1	6	12	3
	10	1	3	4	1	8	1	0	3	6	12

and in those with H after intervals of sleep, that were not present in the experiments with H after intervals of waking. Frequency—as we have already seen—will not account for it, because that factor, so far as we were able to determine, was eliminated by our experimental conditions; and even if it were not entirely eliminated it could not, in view of the similarity of Mc's results in sleep and waking, account for the difference between H's results in sleep and waking. The solution of the problem must be sought elsewhere.

A casual inspection of the data showed that the *O*s very frequently, though they were not constrained by the instructions to do so, reported the syllables in the order in which they were exposed. To ascertain whether there was any relation between this form of report and the report of the initial syllable, we cast our results in sleep and waking, separately for each *O*, into the form of correlation tables. These are given in Table II.

In making these correlation tables, we divided the syllables in *O*'s reports into five groups, accordingly as they were reported first, second, in the middle, next to last, or last. A more extended classification was not practical, since the average number of syllables reported by H and Mc respectively was only 4.1 and 4.4.[16] When 6, 7, 8, or more syllables were reported, the first, second, next to last, and last syllables reported were entered under those headings, and the others under 'middle;' when only 4 syllables were reported, under 'first,' 'second,' 'next to last,' and 'last'—no entry was made under 'middle;' when only 3 syllables were reported, under 'first,' 'middle,' and 'last;' when only 2 were reported, under 'first' and 'last;' and when only 1 syllable was reported, it was entered as 'first' or 'last,' according as it fell within the first or last half of the stimulus-series.

Each *O* gave 4 of these single reports. All of H's cases, given in the waking experiments, fell within the first half of the stimulus-series—1 case at position 2, 2 cases at position 3, and 1 at position 5. Mc's single reports, likewise given in the waking experiments, were equally divided between the two halves of the stimulus-series—1 case in each of the positions 1 and 3, and 2 cases in position 10.

The coefficient of correlation between the stimulus- and recall-series, as computed by Adams' adaptation of Pearson's products moment method,[17] are, for H and Mc respectively, 0.37 and 0.38 for recalls after intervals of waking, and 0.72 and 0.77 for recalls after intervals of sleep. These coefficients, however, are not as significant for our problem as the distribution of the two arrays. If the arrays are studied from that point of view it will be noted that in the experiments in which the 'primacy effect' is evident the first syllable is most frequently reported 'first.' Mc, for example, recalls in the 'sleep' experiments the first syllable 'first' 24 times, once in the 'middle,' and once 'last.' Moreover, he recalls the first syllable 'first' 3 times as often as all the syllables in the other nine positions combined. He shows, in this series of experiments, a pronounced tendency to report the first syllable 'first.' A similar tendency is shown in his 'waking' experiments and in H's 'sleep' experiments.

The conclusion, which we draw from these results, that the 'primacy effect' is to be explained by this 'initial set' towards the first syllable, *i.e.* by the tendency to report the first syllable 'first,' is corroborated by the negative findings in H's 'waking' experiments. In this series of experiments, in which the 'primacy effect' was not obtained, the initial syllable was not reported 'first' as frequently as the second or third in the stimulus-series, nor more frequently than the fourth or fifth. There is no evidence of a 'set' or tendency to report any one position more frequently than any other, and the results verify this observation.

The only way, of course, of determining the accuracy of our conclusion is to appeal to introspection—which we are in the present instance unable to do; but since we can show that it explains the results which have thus far been reported, we think it very plausible. The serial mode of presentation will suffice, we believe, to arouse an 'initial set' toward the first syllable, and thus accent it and cause it to be reported more frequently than any

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[16] *Op. cit.*, 609.

[17] H. F. Adams, An easy method of determining the coefficient of correlation, *Psychol. Bull.*, 15, 1918, 456-459.

other syllable within the stimulus-series.[18] In all the experiments upon this problem up to those of Thorndike, the stimulus-series have been presented serially; and, with the exception of the results of Welch and Burnett, the effect of primacy has always been noted. The single exception strengthens rather than weakens our thesis; for as long as the experiments in the Bowdoin laboratory followed the usual serial procedure, the primacy effect was noticed; but when instructions were introduced that cut across the tendencies usually established by the serial presentation, very different results were obtained. The differences cannot be explained, as we have shown above, by the presence or absence of the factor of rehearsal, but they can be explained in terms of our conclusions as follows: Welch and Burnett's instructions against the rehearsal of the early items not only destroyed the 'initial set' toward the first item that is usually aroused by serial presentation, but, in addition, they established an 'initial set' toward the last item. If this explanation is true the strongest connection would be with the last item; and this item in accordance with Thorndike's corollary would show its strength by being reproduced most frequently and most frequently first—and these are just the results that Welch and Burnett report.[19]

As we have already discussed Thorndike's experiments, we shall not return to them now; but aside from what we have said, we should, since he presented his materials in a random order, expect his results not to show the 'primacy effect.' If our conclusion is correct, a random presentation, everything else being equal, would always give negative findings.

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[18]The efficacy of 'initial set' was tested in a short series of class experiments in which the *S*s were instructed to "report the 4th [5th, 6th, or 7th] syllable in the stimulus-series first." The syllable occupying the position to be named first was invariably reported more frequently than the syllables occupying the other positions—primacy and recency included. A result that any one could have predicted! Surely! Its importance to us is that it is a result contingent upon the 'initial set'—upon the tendency to report first, and it is just this factor, differing perhaps in degree but not in kind, which we believe is established by the serial presentation of the stimulus-series and is responsible for the alleged 'primacy effect.'

[19]*Op. cit.*, 398.

APPARATUS

AN INSTRUMENT FOR MEASURING THE BREADTH OF THE PUPIL

By C. E. Ferree and Gertrude Rand, Bryn Mawr College

There has long been a need in laboratory and clinic work for a convenient and precise means of measuring the size of the pupil and of studying its reactions. The importance of increasing our knowledge of the normal size of the pupil, its pathological deviations, and its normal and pathological reactions is too well recognized to require much attention here. It will be sufficient perhaps to point out that there is scarcely a function of the eye in which the pupil does not play an important part and scarcely a functional test in connection with which the pupil is not an important variable factor. The meager knowledge which we have of the pupil's reactions and of their effect on the various visual functions can not, therefore, be attributed to a lack of incentive for investigation. Our own research program, for example, contains a long list of problems, work on which has been delayed or not even begun because of the lack of an instrument which has the desired precision and flexibility of use.

Some of the essentials of such an instrument may be noted in passing. (1) Because of the diminutive size of the pupil, measurement should be possible to fractions of a millimeter within a small percentage of error. (2) The method of making the measurement should not reduce the amount of light entering the eye, or in any other way disturb the reactions of the pupil to the conditions being studied. (3) It should be possible to continue the measurement without interruption through a considerable period of time. (4) Measurement should be possible over a wide range of intensities of illumination from low to high. (5) The instrument should be easily and readily adaptable to a wide range of conditions to use.

The pupillometer to be described fulfills the above requirements to a reasonably satisfactory degree and is extremely simple in principle and construction. It consists of a telescope provided with a measuring scale extending diametrically across the field, and the auxiliary devices for adjustment needed for its precision and convenience of use. The telescope is furnished with a 12.5 cm. achromatic objective, 3.2 cm. in diam., and with a Ramsden eye-piece. The measuring scale is ruled on a disc of thin glass, 70 divisions to the centimeter. This scale is located in the image plane of the objective lens and is therefore in the same plane as the image of the pupil to be measured. Image and scale are thus magnified the same amount by the eye-piece and the breadth of the pupil can be read directly in terms of scale divisions. The telescope is provided with a range of focus which permits of its use at distances ranging from 25 to 200 cm. The scale must of course be calibrated for the distance at which the instrument is used. This can be quickly and easily done by viewing a millimeter scale at the distance chosen and determining the relation between the two scales. In order to give a high visibility to the scale divisions as seen against the dark pupil as background, an independent source of illumination is needed for the scale. The illumination of the scale is provided for as follows. A straight edge is ground on the disc at one end of the scale. This edge is illuminated by a miniature lamp inserted in the tube shown in Fig. 1. The light passing through the glass between its two surfaces and specularly reflected from the scale divisions, renders these divisions luminous and plainly visible without impairing the distinctness of the image of the iris and pupil. In order that the illumination provided for the scale may not be too high when the pupil is viewed under a low light and conversely, this lamp is connected in circuit with a rheostat the coil of which

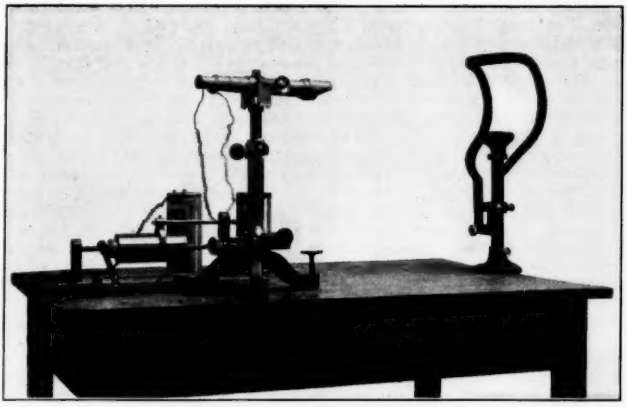

FIGURE I. PUPILLOMETER

rotates under the contact to give small changes of resistance. The mount containing the scale is also made to rotate. This provides for the measurement of the pupil along any diameter. If viewed from a position slightly to either side of the line of sight, the vertical diameter of the pupil should be measured; if viewed from a position above or below the line of sight, the horizontal diameter is measured.

Often in the use of a telescope of this type the visibility of the object viewed can be increased considerably by shielding the objective lens from extraneous light, particularly when there is a strong component of illumination in any direction as occurs, for example, in overhead lighting. A shield for this purpose, shaped from a section of tubing and blacked on the inside, is shown in the figure. It is held in position by a ring at one end which slips over the end of the tube of the telescope. This shield can be rotated to cut off the light from any direction and can be attached and removed at will.

For the convenience of use of the instrument the following adjustments are provided. The stem supporting the telescope is screwed to a plate which can be moved to right or left in a grooved housing by a triple-pitch screw, shown in Fig. 1. This housing is in turn fastened to a plate which can be moved forward or backward in a similar housing by a second screw. The first of these adjustments provides for the placement of the scale on the image of the pupil at divisions convenient for reading, and for following slight movements of the eye to right or left; the second, for the precise regulation of the distance of the telescope, set for a fixed focus, from the eye. The telescope itself is mounted in a collar provided with two short studs or bosses extending out from either side. These bosses rest in adjustable friction bearings in the U-shaped housing shown in Fig. 1. This type of mounting permits of tilting the telescope up or down, as may be desired, in viewing the pupil from below or above. The telescope can be raised or lowered in the usual way by means of a rack and pinion. Finer movements are provided for by a levelling screw as shown in the figure. By means of these adjustments the instrument can be used in a variety of positions without interfering with the use of the eye or with the reactions of the pupil to the conditions under which the eye is being used.

The instrument is simple and inexpensive to construct. We have found it to be a very valuable addition to our optic and ophthalmic equipment. Of so great a value is an instrument of this type that it has been our hope that the measuring scale and the device for its illumination will be added to the telescope which at present forms one of the auxiliary attachments to the perimeter which we have described in earlier papers.

The expense of this addition would be slight and the pupillometer would constitute a valuable adjunct to a perimeter, particularly in the study of pathologic cases. Size of pupil is also one of the factors which makes perimetric determinations differ from individual to individual in case of non-pathologic eyes. That is, it is one of the factors which should be specified in forming a diagnostic scale and be allowed for in rating a case in terms of such a scale or other device used for classification. A foundation structure similar to a perimeter would be of great advantage too in furnishing the conditions and controls required for many of the purposes for which a pupillometer is needed. The details of the combined instrument—its needs and advantages—will be discussed in a later paper.

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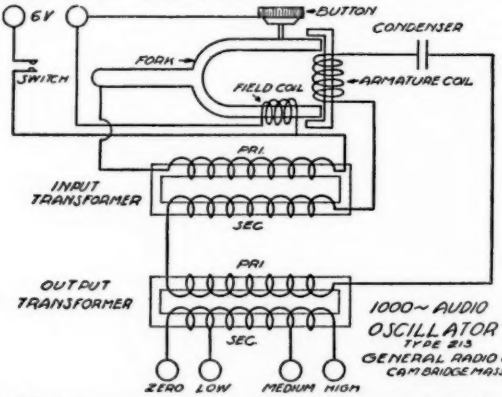
[1]C. E. Ferree and G. Rand, A new laboratory and clinic perimeter, *J. Exper. Psychol.*, 5, 1922, 46-67; Some contributions to the science and practice of ophthalmology, *Int. Cong. Ophth.*, 1922, 488-525; Further contributions to the science and practice of ophthalmology, *Trans. Ophth. Soc.*, 45, 1925, 815-847.

THE AUDIO-OSCILLATOR

By H. M. Halverson, Yale University

For some time the writer used in the laboratory at the University of Maine audio-oscillators for the production of tones of medium pitch (600-1400 d. v.). These tones may be sustained indefinitely without change of intensity and are practically free from harmonics. A note on the instrument for the information of psychologists therefore seems desirable.

The audio-oscillator is essentially an electric tuning-fork and two transformers (see diagram). Unlike the ordinary electric tuning-fork, this fork is operated noiselessly without a spark gap by a microphone button (marked 'button' in the diagram), which is vibrated by one prong of the fork and varies the resistance without a spark and its attendant difficulties. This circuit passes around a coil that encloses the other prong of the fork with enough air-space to permit vibration. A hooked armature is placed about the ends of the prongs, and it is possible that the fork would vibrate thus without further connections, with the armature simply completing the magnetic circuit created by the field.



Actually, however, the armature is itself magnetized electrically. The primary coil of the 'input transformer' is shunted about the fork, and hence carries a current fluctuating as the microphone current fluctuates. The secondary circuit of this transformer is wound about the armature to produce opposite polarity with the fork, and, since the fluctuations are synchronous with the fluctuations in the field, the action is enhanced. Capacity is introduced into this circuit in order to establish electrical resonance for the frequency of the fork.

The secondary circuit of the 'input transformer' is also the primary circuit of the 'output transformer.' From the secondary circuit of the output transformer four leads pass to binding-posts, making possible three voltages and therefore three intensities in the telephone receiver that can be attached.

The instrument is best used with a receiver, although the fork without the receiver gives a fairly loud tone which can be heard for 25 ft., but which can not be varied in intensity. If it is especially desired to omit the receiver, the sound can be satisfactorily conducted to ear-pieces through a rubber tube if the end of the tube is placed near one tine of the fork.

This instrument is the audio-oscillator, type 213, manufactured by the General Radio Company, 30 State St., Cambridge, Mass., and described in their Bulletin 713, January 1925. It is ordinarily made for 1000 d. v., and comes in a cubical box about 6 x 5 x 5 in., weighing 4½ pounds. The stock article is sold for $32, but the manufacturers can make to order frequencies from 600 to 1400 d. v. at little additional cost. It is obviously possible to have more leads brought out from the 'output secondary,' so that a larger number of intensities can be obtained.

The primary circuit requires a storage battery of 4-8 volts.

ELECTRICAL CONTROL WITH THE METHOD OF CONSTANT STIMULI

By J. M. McGinnis, University of Maine

It is well known that judgments in the method of constant stimuli are made quickly and that the progress of an experiment is limited principally by the time required to present the stimuli. The rotating table of Urban and Fernberger was designed to facilitate presentation, and also to reduce the time-interval between the standard and the variable stimulus. It is well, however, to call attention to the fact that many stimuli can be presented electrically and that an electrical control with the proper switches is a very convenient and efficient method of presentation.

The writer has been using such a control for the presentation of pairs of auditory intensities, established electrically in telephone receivers. He has found that a very satisfactory arrangement consists of two circuits, one for the standard and one for the variable. The circuit for the variable includes a rheostat with point contacts, so that the setting for the variable is made simply by moving the arm of the rheostat. The circuits are respectively connected with the mercury cups of the metronome, which, when thrown into circuit, presents the standard and the variable successively with a constant time-interval determined by the rate of the metronome. By inserting between the metronome and the two stimulus-circuits a double-throw double-pole switch, diagonally cross-connected, or any similar commutator familiar in the technique of the reaction experiment, it is possible, by throwing the switch, to reverse the time order of the variable and the standard. The two sides of throw of the switch can be labelled 'variable first' and 'standard first,' and the changing of time-order can be made readily and noiselessly.

By this arrangement it is possible to obtain approximately 200 judgments from an observer in a 45-minute session.

It is obvious that the arrangement is adaptable to any experimental situation where the stimuli can be presented electrically. If the variable changes with discrete stimuli (instead of electrical resistance), a multi-point switch may be substituted for the rheostat. Any time apparatus that will give successive contacts may replace the metronome, *e.g.* a pendulum or a rotating drum with contacts.

In the experiment mentioned a circular radio switch with 7 contacts was used. A spring under the shaft makes it possible to lift the knob in turning, so that the setting of the switch is noiseless and does not afford secondary criteria. It seems probable, however, that slightly more uniform contact will be obtained by a heavier dial switch such as used on resistance boxes.[1]

The principle of the commutator switch is well known. The metronome leads pass to the knife blades and the two circuits are taken off from two points at the one side, each of which is connected diagonally to the opposite point.[2]

[1] Such a switch is part of 'Reaction Apparatus,' catalogue no. 23301, C. H. Stoelting Co., Chicago.

[2] A convenient form is the 'Block Handle Pohl's Commutator,' Knott and Co., Boston.

BOOK REVIEWS

Cosmic Evolution. By JOHN ELOF BOODIN. New York, The Macmillan Co., 1925, pp. xi, 484.

It is probably due to the theory of relativity and the remarkable discoveries of the new physics that there has been so great a revived interest of late in philosophy. Mathematicians and physicists are, after the manner of philosophers, actually studying the universe as a whole, seeking some principle of unity in it. Psychologists, too, no longer pride themselves on limiting their studies to mental phenomena. They are trying to find out what intelligence *is*, or even what the mind *is*, and have made remarkable progress in this hitherto philosophical task.

The new book by Professor Boodin supplements his *Realistic Universe* by a more explicit attempt to do just these two things—namely, to get some unitary conception of the world as a whole, and to bring together some definite conclusions respecting the philosophy of mind. A further section of the book deals very clearly with the theory of relativity for non-mathematical readers.

As regards the mind and its relation to the body, Professor Boodin's book recalls to us how far we are getting from all the old interaction, double-aspect, and parallelistic views. What we have is not mind and body, but a *minded body*, a minded organism. "Mind and body," suggests an artificial bifurcation. Mind "marks a new level of organization, a new quality of behavior, a new pattern control in which the whole organism functions differently from the levels of reflex and habit." "Meaningful behavior is a new level of organization."

But all of this, it appears, is just as far from physiological materialism as it is from psychological idealism. If it is a question of reality, mind is quite as real as matter. "The mental level superimposes a new set upon the whole hierarchy of physical mechanisms." "The physiological organism with its hierarchy of structure does not function in the same way when it is in integral relation with the mental level as when it is dissociated from that level." Minded behavior is thus a new actuality, "a creative addition in the course of evolution of a new pattern control, a new energy system, not resolvable into the categories of physico-chemical, nor into those of neural systems."

When Professor Boodin gets into his metaphysical swing with his ambitious cosmical system of hierarchical control, the going is not quite so easy. Yet his sweeping guess at the method and meaning of the universe is also interesting. The universe is a sort of harmonic system in which every part is in integral relation with every other; the lower levels being controlled by the higher as much as the higher by the lower. We wonder how matter can enter into higher and higher forms of organization. We wonder at the creative synthesis which is somehow displayed. But this is all "due to its moving within the field of cosmic control, to its interaction with the larger universe." Cosmic evolution is not due to any fortuitous concourse of elements but is ever under the control of the cosmic field.

Reality is "a rhythm of multiple histories, which exist at different levels and which interact." There is therefore a cosmic guidance of evolution which is "the incarnation of the energy patterns of the cosmic environment into material bodies." If there is an evolution towards life, mind, goodness, beauty, divinity, it is because these are all active throughout the cosmos, stimulating every part in their own direction. Thus "the world as we know it is a pluralistic world of multiple energy patterns, histories, and levels."

So it comes about that neither materialism nor a psychological idealism do justice to the complexity of the world. "The claims of idealism are

given due recognition if it is shown that life, thought, the sense of beauty, communion with God, are eternal and intrinsic aspects of the whole of reality and that the higher levels exercise a measure of guidance over the lower, while they also require the lower for their realization."

University of Iowa G. T. W. PATRICK

Biological Memory. By EUGENIO RIGNANO. Translated with an introduction by E. V. MacBride. New York, Harcourt, Brace & Co., 1926, pp. vi, 253.

"The final result of all action is really present from the beginning in the form of a mnemonic accumulation: the medium, or the environmental relations towards which the animal strives, operate now like a *vis a fronte* solely because they have formerly been a *vis a tergo*, and because the physiological activities which they have then aroused in the organism have left behind them a mnemonic deposit, which now acts like a veritable *vis a tergo* in moving the living being. Every manifestation of purposefulness, from ontogenetic development, which builds up organs which can only be used in the adult state, to the property of physiological states in general, corresponding to certain environmental conditions, to become active at the first appearance of phenomena which ordinarily precede, but do not constitute, the whole of these environmental conditions; from the perfect morphological adaptation of the organism to the environment, completed before the environment has been able to exert its formative action, to the wonderful arrangements and specialized structures so nicely calculated in view of certain probable conditions to which, in the future, the organism may be exposed; from simple reflex actions 'mechanized' for the purpose of the preservation and prosperity of the individual to all the most complex instincts by which animals provide beforehand for future conditions of which they know nothing: all these purposeful aspects of life can be explained as so many manifestations of mnemonic nature."

The above quotation contains the gist of the thesis (and exhibits, as well, the style of the translation) which Rignano elaborates in the present volume. The work may be considered as an extension and clarification of positions already developed by the author in his books, *On the Inheritance of Acquired Characters* and *The Psychology of Reasoning* (English translations 1911 and 1923 respectively).

Biologists have been confronted for a long time by three dilemmas. (1) Is development effected by preformation or epigenesis? (2) Is the germplasm composed of preformistic gemmules or is it made up of substances which are altogether devoid of the power of representing and of determining each a single morphological character or a special process of development? (3) Is there specialization of nuclei or is nucelar division qualitatively equal? Many important and impressive experimental facts may be adduced in support of both sides of all three of these questions—hence the dilemma. But since—in view of the factual evidence—each side of the different questions is about equally tenable, Rignano suggests that the problem has been wrongly stated, or, what is more likely, that the dilemmas are really nonexistent. The facts on both sides should be envisaged in the framework of an hypothesis which shall be able to resolve the apparent conflict. Such an hypothesis is the one which Rignano presents under the name of the centro-epigenetic doctrine.

Centro-epigenesis tries to meet the above dilemmas by postulating (1) the existence of a 'plasmatic' or nervous energy which proceeds from a special part of the organism (central zone of development); (2) the presence in the germ-plasm of a number of specific accumulators which are capable by discharging themselves of giving rise to specific deposits of energy and which become active one after another in a definite sequence from the beginning of development until its completion; and (3) the accretion to nuclei

outside the central zone of elements of specific potential different from the original ones. These processes of nervous action and accumulation are related to mnemonic phenomena, or more strictly, all mnemonic (in the narrower sense) and vital phenomena are special cases of nervous action and accumulation, which are mnemonic (in the most general sense) in nature, in that each discharge gives rise to a nervous current of a definite specific character and that the substance which produces this current is the same substance which this current can and does in its turn deposit when acting as the current of charge. This property confers on the accumulators a true mnemonic nature.

With this hypothesis as a working basis, Rignano proceeds to a treatment of the mnemonic character of the purposefulness of life. Every organism strives to maintain a physiological equilibrium or to restore it every time it is disturbed by a change supervening in either the external or internal environment. To each factor in the environment which is capable of preserving or restoring the normal condition there corresponds an impulse of 'longing,' 'desire,' 'attraction,' etc. To these states of the organism Rignano applies the term 'affective tendencies.' The primary affective tendency has as its function the maintenance and restoration of the physiological equilibrium. The author gives an elaborate account of the manner in which the number of environmental factors which serve to maintain a normal condition of the organism is ever increasing, and of how factors towards which or away from which the organism strives operate like a *vis a fronte* solely because they have formerly been a *vis a tergo*. The secondary affectivity serves as a check upon the primary in order that the latter may not lead the organism into harmful situations. The proper balance between these two affectivities results in a caution which is beneficial in maintaining the most satisfactory relation with the environment. The same affectivities are made use of by the author in explaining higher mental activities in man, such as attention, reasoning, and intelligence. Reasoning, for example, turns out to be a series of inter-connected experiments *conceived*, but not actually performed. If they were performed they would conform to the operation of the two affectivities, the primary one urging the individual on to discoveries which shall bring a satisfactory *mental* equilibrium and the secondary one acting as a check upon the primary in order that errors may be avoided.

The psychologist will find the latter part of the book, together with the author's *Psychology of Reasoning*, of very great interest. Attempts to account for higher thought processes in terms of mental sets, neural dispositions, and other vague determinants are familiar enough. Here we have an effort to follow the lead given by these vague phrases. Rignano has started with simple principles derived from the observation of the characteristics and behavior of lower organisms and has given a consistent picture of the evolution of behavior and consciousness within the framework of these same principles.

Harvard University C. C. PRATT

Mind and Its Place in Nature. By DURANT DRAKE. New York, The Macmillan Company, 1925, pp. xv, 259.

The work, written from the viewpoint of *critical realism*, is epistomological in that an attempt is made to answer questions concerning the possibility of knowledge under the general limitations of conscious states, and metaphysical in that a theory is set up for the nature of mind in a setting of objective reality. The author holds that the denial of the existance of an "outer" world is no more justified than the denial of other minds, that there can be no logical evasion of solipsism if we abandon the existence of an objective reality. The author attempts to show in what way and under what limitations consciousness might be considered as knowledge of

an objective reality. He holds that there are certain aspects in the sequence of mental life which warrant the assumption of controlling conditions from an objective world. The fact that part of our mental operations are susceptible to the knower's will, and other parts proceed on their course in spite of the knower's will, is one suggestive fact. The coherence and predictability of certain aspects of mental life, which reaches a precise organization in spite of the known laws of perceptual illusion, warrants the belief that certain phases of conscious processes constitute knowledge about objective reality. Our knowledge of other minds is just as firmly limited to spatio-temporal terms as our knowledge concerning material objects. The fact that some other observer sees my mind only in space and time factors does not in the least prove that my consciousness, which is unobservable to him, does not exist.

The author seems to object to the pragmatic test as the ultimate determination of belief in objective reality. He says, "It would not be accurate to say, as some pragmatists have said, that verification made our beliefs true. If they were true at the time of verification, they were true before the verification, and would have been true if they had never been verified." In the face of this apparent denial of the pragmatic test Professor Drake seems to rest the validity of his theory of mind on the fact that it works. It is difficult to see in what way he has escaped the pragmatic test after all. It would seem that he has drawn too sharp a line between belief and the test for belief, a line which probably had its origin in the precise demarkation of the various parts of the reasoning process by the older logicians, and which has been abandoned by the modern psychologists writing on the actual nature of reasoning. The pragmatic test was written by a psychologist. The smooth and successful operation of the mental processes, accompanied by pleasant feeling-tone, is *both* a condition for belief and *at the same time* the pragmatic test.

For the psychologist, interested in the functional point of view, Professor Drake's theory of consciousness as a function of an objectively physical organism, a qualitative aspect of an objective world otherwise measurable only in spatio-temporal terms, is amply satisfying. He has considered, in a more adequate manner, the facts of psycho-physical relations and has presented a really valuable contribution by showing that the philosopher has never been free from psychological determinants.

University of Michigan ADELBERT FORD

The Language and Thought of the Child. By JEAN PIAGET. Translated by M. Warden. New York, Harcourt, Brace & Co., 1926, pp. xix, 246.

Piaget here reports the results of a study of the language, conversations, explanations, and questions of children. The methods used are all modifications of a basic plan consisting in taking verbatim reports of the conversations of children under natural conditions. The author is a careful student and displays great ingenuity in classifying his results and in finding general principles exhibited by them. The reader will be pleased also by the fact that Piaget is not given to the weakness so rampant among psychologists of today which consists in building up personal systems with a lordly disregard for the results of others. He, a biologists of distinction, is versed in 'knowledge about knowledge' and exhibits a caution which one would expect to find only among veteran students of syllogistic thinking and of epistemology.

At first though it might seem a hopeless task to bring order out of the chaotic talk of children. Piaget, however, is ingenious. His primary principle, as Claparède points out in an introduction, is the theory that the thought of the child evolves from an ego-centric and autistic type to the logical and socialized type of adulthood. His facts certainly support his principle. Analysis of the sentences of the 6 year old child shows dominance

of ego-centric speech of which he finds three types: (1) repetition (echolalia) a "remnant of baby prattle" in which the child repeats words and phrases oblivious to hearers and to meanings; (2) monologue, "the child talks to himself as though he were thinking aloud," he addresses no one; and (3) dual or collective monologue, "where an outsider is always associated with the action or thought of the moment but is expected neither to attend nor to understand." Of socialized speech, Piaget enumerates 5 types. Formulae are also developed to measure the index of ego-centric language and to give the index of understanding.

Chapter I is a detailed study of 2 children. In Chapter II Piaget's principal thesis is confirmed by the results from 20-odd children between the ages of 4-7 years. In Chapter III, a study is made of 30-odd between the ages of 6-7 years regarding their ability to understand one another. One child is given a story or an explanation of the workings of some simple mechanism to repeat to another. A study of hundreds of these reports reveals the ego-centric character of the child's thought. The child fails so miserably in communicating what he understand because he disregards the 'how' of phenomena, substituting for it some autistic schema of his own. To become mentally mature is to realize that thought must be conveyed in categories which others can understand. This does not appear in the child until about the age of 8 years.

A proverbs test given to 70 children (boys and girls, 9-11 years of age) exhibits 'verbal syncretism,' in which a child having one proverb in mind finds a 'corresponding' one by fitting both into a semi-archaic schema of his own. These results especially are wonderful devices for exhibiting the intimate workings of the child's mind. If the child does not understand the proverb, that makes no difference to him, he must justify himself, so he pairs proverbs on the basic of the most bizarre and unseeming associations of verbal resemblance. As usual Piaget relates his findings to attained facts, *e.g.* Decroly's success in teaching words before the alphabet, Bergson's criticism of associationism, and the results of the linguistic studies by Schuchart, Lelande, and Cousinet.

Wells College C. O. WEBER

Note Sopra la Originalita del Pensiero. By LEONE VIVANTE. Rome, P. Maglione and C. Strini, 1925, pp. 295.

From the theoretical point of view, especially in connection with the ever-recurring problem of the mind-body relation, this book makes a valuable contribution. As has been the usage in our own language, the author assumes under the term "thought," a great variety of conscious activity, somewhat in the sense of James's "stream of thought" or his still wider use of the word "feeling." This is, however, not the best use in present-day experimental psychology, but theoretically permissible in books of semi-psychological persuasion. The specific problem which confronts the author is the best explanation of a 'free' mind that is at the same time conditioned by a physiological system. The initial cause of mental processes is an original synthesis which assimilates to itself either initially or progressively in time a series of material conditions that become intentionally regulative but never constitutive. Externally these conditions appear to be causal, but in reality they are only secondary though essential concomitants. The doctrine is a form of idealistic monism. The synthesis is experienced directly, therefore it is not an abstraction, The problems of time and space afford an unfolding of the true essence of consciousness. Often the medium through which consciousness expresses itself whether it be the body itself or a product of bodily movement like language is quite inadequate to the reality of consciousness that lies behind it. Detailed examples of this fact are given throughout the book especially from the poetic literature of Italy. While the presentation of his point of view

labors somewhat at times and at other times appears more romantic than scientific in attitude, this difficulty in making his experiences or "thoughts" clear may itself be the best illustration of the point that he is trying to make. No doubt, too, the double loss of meaning through translation into another language still further obscures the issue. On the whole the book shows much serious preparation for the task in hand and many novel insights into the problem.

University of Iowa CHRISTIAN A. RUCKMICK

Das Stottern (Assoziative Aphasie). By DR. EMIL FROSCHELS. With 12 figures. Leipzig and Vienna, Franz Deuticke, 1925, pp. iv, 134.

The author secures his evidence on the nature of stuttering and stammering from clinical cases secured in private practice. After reviewing the various theories in relation to symptoms he openly disagrees with those of his associates who have previously held that such speech defects are to be considered as a disturbance of the reflex centers. The prevalent classification of stammering disturbances into tonic and clonic types is not supported, he says, by clinical examinations. Often the patients exhibit a mixture of both tonic and clonic convulsions, or may change from one to the other without warning in successive disturbances. The same subject, repeating a given sentence over and over, does not repeat the same type of stammering movement, but shows constant variation in the form of the stammering convulsion. Patients exhibit a tendency to avoid certain particular words consistently, to invert the order of words in sentences containing these difficult words, and to show the motor abnormality most when they are conscious of being observed by friends or other listeners. When the subject can be induced to forget his self-consciousness the abnormality quite often disappears.

Dr. Froschels says he follows a suggestion of Hoepfner that the motor manifestations to be observed in stuttering are to be considered as an abnormal functioning of the voluntary controls rather than a disturbance of the reflex centers. The diffuse motor concomitants are to be classed as willed acts and the origin of the speech defect is in the higher centers more directly concerned with the manifestations of consciousness. The cause of the disturbance goes back to atactic conditions in the learning of the motor coördination of speech and becomes aggravated by affective elements—shame of displaying the abnormality before the social group. Associative inhibition is established between the idea of the movement and the thought expressed by the words to be uttered. In this conflict repressions occur for a portion of the words in the patient's vocabulary and this accounts for the consistent avoidance of certain particular words. A review is given of the contemporary methods of curing stutterers by means of exercises, rhythmical stimuli, etc., and this is followed by the author's psychoanalytic method of diagnosis and treatment.

University of Michigan ADELBERT FORD

Crime and Custom in Savage Society. By BRONISLAW MALINOWSKI. New York, Harcourt, Brace & Co., 1926, pp. ix, 132.

This book is a field worker's study of the laws and customs of the people living in a single ethnographic province—north-west Melanesia. These Melanesians inhabit an archipelago of flat coral islands which lie to the north-east of New Guinea. Malinowski has the virtue of reporting details and of illuminating his facts with a constant fire of ju icious evaluation. He does not abjure theory, but he does criticise the academic tendency to simplify the account of savage life. This tendency expresses itself, for example, in the wide-spread notion that the life of the savage is dominated

by automatic slavish, and unwitting submission to tribal custom and taboo, the view supported in the writings of Hartland, Rivers, Lowie, and others. The author's study of the Melanesians is of great psychological interest. He shows that these savages live in a surprisingly complex network of law, motive, inducements, and evasions. It is in vain that any one should seek to make routine machines out of them, moved by only one or a few drives. He demonstrates, in the major incidents of their lives (marriage, ownership of property, religious and family duties, death, suicide, crimes of other sorts, mourning, etc), that the notion that their life is dominated by mere gregarious instinct and unwritten communism is mistaken.

A superficial view might lead to the notion that ownership of property is communistic. Closer examination shows that, on the contrary, there is marked individual ownership. To listen to the testimony of these savages, one might well get the idea that mother-right, the unwritten tribal laws regarding incest, and so on, are things they would never think of violating, but, considering their practices, we find recognized, sophisticated, and effective subterfuges and evasions for violating these confessed philosophies. Far from being like a communistic hive of bees, individualism is rampant, the ego-centric element manifesting itself in the struggle to excel in wealth (the ownership of yams), in generosity, in sexual prowess, etc. Suicide among these people, is often merely an inability to endure life because of the memory of personal insults. Malinowski represents these savages neither as the care-free children Rousseau describes, nor as the hide-bound slaves to custom that Havelock Ellis portrays, but as people very much like ourselves, with constant friction between the demands of individualism and the demands of communism.

Wells College C. O. WEBER

The Physiology of Mind: An Interpretation Based on Biological, Morphological, Physical, and Chemical Considerations. By F. X. DERCUM. Philadelphia, W. B. Saunders Co., 1925, 2nd. ed., pp. 287.

In the preface the reader is informed that "the object and scope of this book is to present the basic facts of those reactions of the organism to the environment which under given conditions manifest the qualities which we speak of as 'mind.'" Physico-chemical processes are said to account for reactions called mental; the assumption of a psyche is unnecessary. "In any event psychology can only be regarded as a department of brain physiology."

In the early chapters the author presents in a simple and direct manner some of the more important facts regarding the development of the nervous system from the diffuse to the synaptic type. Receptors have been differentiated to respond to specific types of stimuli and protoplasm is said to be 'transparent' to many forms of energy. Stimulation is an automatic and mechanical process and transmission of excitation along the neurones is electrical in nature. The hypothesis that the neurones move is discussed.

The treatment of consciousness is generally too dogmatic and speculative to be of abiding scientific worth. Terms such as sensation, perception, thought, consciousness, etc., are used without definition and the great mass of factual material which constitutes modern experimental psychology is almost wholly ignored. Physical and mental concepts are jumbled up together and their relationship to each other is left in obscurity. If one would write a physiology of mind, one needs first to examine critically the phenomena called mental and to present adequately the facts whose physiological interpretation is desired.

After discussing consciousness Dercum returns to the more objective material but consciousness remains as a fly in the ointment. Loeb's theory of the tropism is accepted but in discussing instinct consciousness is invoked as an explanatory principle. A chapter on pleasure and pain presents a

little speculation in which pain is said to be related to the destruction of the neurone molecule. A final chapter entitled "The Nervous System in the Light of Einstein's Interpretation of Energy" points out that living substance is part of the universe and that principles of explanation valid in the physical sciences are probably also valid in biology.

There are two addenda: a chapter on the pathological physiology of mind and an appendix on Freudism. The latter is a clear and brief criticism of psychoanalysis and the 'Freudian mysticism.' The book as a whole is very readable and in many places thought-provoking, but it does not represent any genuine advance in physiological psychology for the reason that its psychological perspective is inadequate.

University of Illinois PAUL THOMAS YOUNG

Learning to Typewrite. By W. F. BOOK. New York, Gregg Publishing Co., 1925, pp. 463.

The first sentence in the preface states that this book "will appeal most strongly to teachers of typewriting and to those preparing to direct learners of this art," and so it will. Although the author elsewhere in the preface suggests that the book might be used in classes in educational psychology "as a text for the intensive study of the learning process," the psychologist will find that it is far too repetitious to serve as a college textbook. By the device of calling the learning process different names, the author goes over and over the same ground. Chapter 1 of Part I is called "Psychological Basis for the Acquirement of Skill;" the same laws are treated in Chapter 2 under the heading, "General Nature of the Learning Process;" an Chapters 3 and 4, the same principles reappear under the captions, "Laws which Control the Origination and Fixation of Habits in the Acquisition of Skill," and "Laws which Control the Selection and Improvement of Habits in Learning;" and finally, Chapters 5, 6, and 7 take up the same process under the title "How Habits are Originated," etc. Thus Part I, which contains 154 pages might easily and profitably have been reduced to one-half or one-third that compass.

Part II, "Scientific Analysis of Learning to Typewrite," is the heart of the book and is thorough and informing. Part III, "The Rôle of the Teacher in Selecting and Directing Learners of Typewriting," is for the most part a repetition from a slightly different angle of the material in Part II. For example, there is little difference in treatment between "Proper Attitudes towards Success" (Part III, pp. 358 ff.) and "Influence of the Learner's Mood and Attitude towards his Task" (Part II, pp. 302 ff.). This re-hashing of the material is typical of the book. This very weakness of inordinate repetition may not be a fault if one considers that it will serve to establish certain habits on the part of teachers of typewriting.

The learning process as described in the book is familiar to psychologists. The principles of habit formation put forth are those that James made popular; Book gives us nothing new. The rôle of will and incentive in learning is repeatedly emphasized. The original experiments referred to, especially those undertaken to determine the voluntary motor ability of the world's champion typists, are exceedingly well reported. The book will be useful not only for teachers of typewriting, but also as a reference for all who are interested in the learning process.

Louisiana State University PAUL CAMPBELL YOUNG

The Measurement of Emotional Reactions: Researches on the Psychogalvanic Reflex. By DAVID WECHSLER. New York. Archives of Psychology, No. 76, 1925, pp. 181.

Dr. Wechsler has enjoyed exceptional opportunities in working both in Paris with Professors Lapicque and Piéron, and at Columbia with Pro-

fessor Woodworth. His book reflects these opportunities. Not only does it provide a clear, complete, and concise exposition of the basic facts concerning the psychogalvanic reflex, but it further supplies much new material secured by the writer from his own experiments in this country and abroad.

In an introductory chapter the author sketches briefly but with ample documentation the development of the methods of expression in the study of emotions, and of the method of the psychogalvanic reflex in particular. In the five succeeding chapters he defines the psychogalvanic reflex, describes the technique used for its measurement, discusses its physiological aspects (conductivity of the skin, etc.) and proposes a theory to account for the phenomenon. The two next chapters are entitled "Psychological Correlations" and discuss the relations between the psychogalvanic reflex and mental processes. One chapter is devoted to clinical and practical applications. A final chapter contains a general summary of the work and some generalizations with respect to the nature of affection and emotion. A bibliography of 120 titles is appended.

In the new material derived from Dr. Wechsler's personal experimentation, particular significance attaches to his results on the relation between intensity of stimulus and intensity of reflex, and those on the specificity of emotional reactions. With respect to the first of these questions Dr. Wechsler finds that "the psychogalvanic reflex appears to vary roughly as the intensity of the affective tone elicited. It does not vary as the intensity of the stimulus, except in so far as increasing the intensity of the stimulus tends to evoke a proportionately greater affective reaction" (p. 166). With respect to the latter of these questions he finds that "results from a number of intercorrelations between various types of stimuli would indicate that individuals tend to be *specifically emotional*, that is, react emotionally to specific types of situations rather than uniformally to all types of situations" (p. 166).

By no means least among the many merits of this book is the fact that it is by an experimentalist for experimentalists. In his description of technique Dr. Wechsler does not confine himself to generalities, but supplies detailed information concerning electrodes, wiring, recording of deflections, etc., which cannot but be of value to anyone undertaking a piece of research which involves the use of the psychogalvanic reflex.

Harvard University J. G. BEEBE-CENTER

Beginners' Logic. By R. H. DOTTERER. New York, The Macmillan Co., 1924, pp. 335.

It is the opinion of the reviewer that Mr. Dotterer has given a treatment of elementary logic somewhat superior to that which commonly occurs. He has included very little irrelevant discussion of psychological matter, and has confined himself fairly closely to a discussion and analysis of logical facts. One is not to be misled by his definition "logic is the science of valid thinking." The book is not about thinking. It is a difficulty with all books on elementary logic that they do not make sufficient use of what is known concerning elementary logical relations. At present the theory of general propositions is in a position to replace the whole of the traditional treatment of propositions, of immediate inference, and of syllogism by a much more satisfactory analysis; yet writers of elementary texts continue to present this theory in its traditional form, much of which is inadequate and false.

Mr. Dotterer has a good introductory treatment of the algebra of classes and of the logic of elementary propositions (Chap. VII), but he is confused when (p. 119) he implies that from the fact that a proposition is false it follows that it is absurd or self-contradictory. Propositions which are self-contradictory are, of course, false, but obviously the converse does not hold.

The latter half of the book is given to a study of hypotheses, induction, and probability. The exercises and illustrations connected with this part of the book are particularly good.

Harvard University — C. H. LANGFORD

Transcendental Values. By R. P. BYERS. Boston, Richard G. Badger, 1925, pp. 69.

If originality of thought and obfuscation of language either separately or conjointly merited the reviewer's attention this monograph would deserve detailed notice here. The author aims to show "the applicability of this great Law [of induction] to ranges of Philosophy that are utterly abstruce [sic]." To avoid the erroneous meanings of words he uses algebraic symbols which "have the further advantage of not having any connotation attached to them." But he has already warned us that they must not "be taken in any algebraic sense!" After repeated re-readings the reviewer is forced to admit that he has not yet attained to the high rank prescribed in the author's closing remarks: "I shall consider myself fortunate if able to describe it [the method] without at the same time creating misunderstanding. For its sphere of final operation is near the margin of consciousness and the successful use of it demands a delicacy of attention unusual to ordinary thinking."

University of Iowa — CHRISTIAN A. RUCKMICK

Children's Drawings. Edited and compiled by STELLA AGNES MCCARTHY. Baltimore, Williams and Wilkins, 1924, pp. 164.

This study by a group of well known educators, composing the Child's Study Committee of the International Kindergarten Union, was undertaken for the purpose of constructing a scale for the drawing ability of children of the primary grades. Since the Thorndike and the Kline-Carey scales are best suited for older children, the new scale fills a need.

As a test it has the great merit of being standardized on the basis of 31,239 drawings from children in various parts of the country. The three most frequently represented objects in children's drawings were used in constructing the scale, and thus the test has the quality of engaging the child in subject of particular interest to him. Moreover, practice is in some measure controlled, since probably all children have had some experience in drawing the particular objects required by the test.

The investigators have taken full advantage of the implications of their data on many questions of particular interest to the art educator. Although conclusions arrived at are often confirmations of previous workers, or notions current about art, there is a certain newness in having these ideas about art supported by the quantitative evidence that is to be found in this book.

Harvard University — T. KARWOSKI

Ueber Begriff und Formen der Intelligenz. By OTTO LIPMANN. Leipzig, J. A. Barth, 1924, pp. 48. Reprinted from *Zsch. f. angew. Psychol.*, 24, 1924, 177-224.

The author presents and discusses a number of definitions of intelligence and stresses the point that a given intelligent reaction to a situation does not necessarily indicate intelligence on the part of the individual reacting, since the response may be purely imitative or learned. According to Lipmann, intelligence depends (1) upon the ability to make *Gestalten*—in the sense of sizing up a complex situation; (2) upon the disposition to make

Gestalten at a given moment; and (3) upon the capacity to make valid and purposeful *Gestalten*. It is really a three factor theory depending (1) upon general ability to adapt to new situations; (2) upon special ability to meet definite situations; and (3) upon specific memory and language ability.

Lipmann criticises the method of averaging scores or mental ages obtained from several tests, and suggests that a profile showing the results is a much more desirable method. The Binet tests are characterized as primarily tests of school adjustment and not tests indicating ability to adjust to new and changing conditions of life.

Lipmann differentiates three types of *Gestalt* formation—*Nachgestalten*, *Neugestalten*, and *Umgestalten*. That is, he distinguishes between the mere reproduction of situations, the creation of new situations, and the arrangement of situations by either or both of the first two methods.

University of Chicago SIMON H. TULCHIN

La Defense Psychique. By I. BOAS. Paris, Félix Alcan, 1924, pp. 253, 18 pl.

In the first part of this book Dr. Boas inquires into the psychical concomitants of the volumic changes recorded with the plethysmograph. His conclusion, based partly on his own data but mostly on data drawn from the literature, is that vaso-constriction correlates not with pain, attention, or mental activity, but with a 'psychical defense,' the chief characteristic of which is to overpower and render innocuous an otherwise harmful perception.

The second part of the book is devoted to a discussion of 'psychical defense.' From the physiological point of view Dr. Boas considers this mechanism to be essentially an inhibition. From the biological point of view it ranks with attack and flight as one of the major defense mechanisms. Its utility resides in the fact that it enables the organism, by delaying and fractionating its reactions, to cope more efficiently with its environment.

In a concluding chapter the author discusses the relation between 'psychical defense' and suppression. Analytic therapy, he believes, in doing away with suppressions may in certain cases deprive the organism of a perfectly normal defense mechanism, and thus sacrifice an ulterior but permanent adjustment for an immediate but temporary one.

Harvard University J. G. BEEBE-CENTER

The Gang Age. By PAUL HANLY FURFEY. New York, The Macmillan Co., 1926, pp. xii, 189.

Furfey presents an interesting study of the pre-adolescent boy in which he displays an understanding of youth culled from intimate contact with its problems. During the period between the age of eight and the onset of puberty boys show a characteristic team spirit that draws them into gangs. Among the various ages in life which invite the research of the psychologist, there is none more difficult and yet more charming than the "gang age." In his sympathetic picture of the needs and satisfactions that throng the experience of the pre-adolescent boy, Furfee adds a valuable chapter to genetic, and social psychology.

Harvard University W. F. VAUGHAN

NOTES

The Accuracy of the Method of Constant Stimuli: A Reply to Dr. Urban*

By courtesy of the editors I have seen Dr. Urban's article in proof and have been conceded a few days for reply. The facts at issue may be briefly and simply set down.

That the reader may have a rôle in the discussion, instead of viewing it as a private debate, let him consider a standard stimulus (S_0) of 100 grm. with which a series of other stimuli (J), ranging below and above S_0, are to be compared, in an effort to find the differential capacities of the organism. That value of J (say 97 grm.) which is reported 'lighter' by an observer just 50 times in 100 is called by Urban S_1; the correlative point for 'heavier' (say 102 grm.) is S_2. If we measure from the standard ($S_0 = 0$), S_1 (= 3 grm.) is the limen for 'lighter' and S_2 (= 2 grm.) likewise for 'heavier.' The question is: *Does Urban's formula give the probable error of these limens?* The argument has three heads: A, B, and C.

Table I

	Observers						
	I	II	III	IV	V	VI	VII
1. S_1	93.34	95.08	97.91	95.44	94.22	95.29	95.80
2. h_1	.1258	.1053	.1389	.1234	.1274	.1102	.1145
3. p.e. (Urban)	3.933	3.142	6.607	4.484	6.635	3.791	4.436
4. p.e. (M) (empirical)	.262	.214	.458	.382	.454	.208	.285
5. p.e. (Md) (empirical)	.328	.268	.574	.479	.569	.261	.357
6. S_2	99.68	99.27	99.39	98.24	97.16	100.75	101.17
7. h_2	.1361	.1109	.1452	.1180	.1157	.1150	.1155
8. p.e. (Urban)	9.477	5.491	8.282	6.232	7.518	5.765	7.385
9. p.e. (M) (empirical)	.259	.198	.299	.367	.632	.371	.375
10. p.e. (Md) (empirical)	.325	.248	.375	.460	.792	.465	.470
11. Range of S_1	7.87	6.28	13.21	8.97	13.27	7.58	8.87
12. ± 1 p.e. (Urban) S_2	18.95	10.98	16.56	12.46	15.04	11.53	14.77
13. Extreme range of S_1	3.96	3.30	5.60	3.53	5.55	2.32	3.11
14. fractional limens S_2	3.61	3.11	4.26	3.98	6.29	4.72	3.88

(A) To clear the air, let us enquire a moment into the nature and function of any probable error. A p.e. is designed to inform us how a given magnitude (the limen) may be expected to vary if we repeat the observations again and again and thus derive a whole set of values instead of only one. Everybody knows that the best way of finding the dispersion (p.e.) of a limen would be to make, say, 100 or more independent determinations under stated conditions and then note how these 100 values are disposed; that is, compute the p.e. of their central tendency. That is ideal; but life is short and work is hard, so we try to infer a theoretical value that will serve our scientific needs and yet will save time and labor. Well-known methods exist for predicting the probable scatter of an average from the dispersion of the individual measures which define it. A theoretical p.e. then fulfills its function only when it forecasts with usable precision the empirical variation of the average to which it applies; any value which fails under this primal test is not a probable error at all, no matter how it may have been derived nor by what name it is called.

*Note. Since time and distance make it impossible to submit this note to Dr. Urban before its publication, I have taken the responsibility of including Dr. Culler's rejoinder along with Dr. Urban's criticisms. The reader will find it convenient to consider both comments at the same time and in the same place.—Madison Bentley

Let us apply this cardinal criterion to the figures which Dr. Urban himself gives and which it is the purpose of his article to defend. Look at Table I.

S_1 here is the limen for 'lighter' (S_0 being 100 grm.) and S_2 likewise for 'heavier' (because of a large time-error, many S_2's are below the standard); h_1 measures the 'precision' of a given *O*'s reports for 'lighter,' and h_2 for 'heavier' ($h = \frac{1}{\sigma\sqrt{2}}$, where σ is the standard deviation of the normal percentile curve as fitted to the observed percentages). In cross-rows 3 and 8 are mustered Urban's values for the probable errors of the several limens; it is these figures which are in dispute and which form the subject of our two papers. In rows 4 and 9 appear the *empirical* p.e. [by the usual formula: p.e. (M) $= \frac{.6745\sigma}{\sqrt{n}}$] for the mean of nine (*O*s I, II, III) or six (*O*s IV to VII) independent values; in rows 5 and 10, likewise, the actual p.e. for the median of the same figures [p.e. (Md) $= \frac{.8454}{.6745}$ p.e. (M) in a normal dispersion]. In rows 11 and 12 we see the range, for each value, of ± one p.e. (Urban); for *O* I, S_1 the figure 7.87 equals twice the p.e. 3.933. In 13 and 14, finally, we see the *extreme* range of the partial limens; for *O* I the lowest S_1 is 91.68 and the highest 95.64, the two being separated by 3.96 grm.[1]

TABLE II

The first figure for each *O* stands for S_1, the second for S_2

	Observers													
	I		II		III		IV		V		VI		VII	
1.	.50	.19	.53	.28	.42	.26	.39	.32	.42	.42	.31	.41	.35	.26
2.	$.26^9$	$.10^9$	$.28^9$	$.15^9$	$.22^9$	$.14^9$	$.21^6$	$.17^6$	$.22^6$	$.22^6$	$.17^6$	$.22^6$	$.19^6$	$.14^6$

What do these figures have to say? If we made 100 separate determinations of a given limen in lieu of only one, Urban would have us believe that about 50 would fall beyond the limits (in I, S_1) of ±3.933 or 7.87; in I, S_2 they would scatter still more and one-half would fall outside a range of ±9.477; and so on.[2] Is there any way of testing this prediction? Happily for our needs, Dr. Urban fractioned his original data, comprising 450 reports on each pair of stimuli,[3] into nine groups of 50 and computed h and S for each; these limens, based on 50 reports, we may call 'partial' or 'sub-limen' and those from the whole number (450 or 300) 'total.' Knowing how much the partial limens actually diverge, we have a useful empirical check upon the accuracy of any proposed theoretical figure. To this end we may use either p.e. (M) or p.e. (Md); since the limen, in this method, is defined as the median of the cumulative percentile curve, we may properly use p.e. (Md); being higher than p.e. (M), these are also more generous to the Urban-hypothesis. Let us compare the p.e. (Urban) and p.e. (Md) in two ways.

(1) In rows 11 to 14 we see how far ± one p.e. (Urban) extends and also the limits without which *every* partial limen actually falls. Were

[1] Rows 1, 2, 6 and 7 are copied from Urban (7, 192), 3 and 8 from *ibid.*, p. 226; rows 4, 5, 9, 10, 13 and 14 were computed from the figures for S_1 and S_2 in Tables 1-7 (8, 276-8). Rows 11 and 12 are obtained by multiplying the corresponding p.e. (3, 8) by two. We may note here that the mean values of S_1 and S_2 in the Tables differ, by a few hundredths, from S_1 and S_2 as recorded in rows 1 and 6; the latter figures are computed directly from the whole number of observations. These trifling disparities have nothing to do, of course, with the argument here. We may observe also that the first number of row 9 (I) is .259. In my earlier paper (2, 85) this was given as .276 from Thomson (6, 52); upon recomputation it proves to be .259.

[2] If the limens are normally disposed, one-half will lie more than one p.e. from their average.

[3] With *O*s I, II and III; six sets of 50 (300 in all) with *O*s IV to VII.

Urban's p.e. reasonably true, one-half of these 102 sub-limens ought to lie beyond the bounds of ± one p.e.[4] But not a single one does actually fall outside; they all cluster far within his theoretical boundaries. Table II brings home the facts.

In row 1 we discover that the partial limens (for I, S_1) are crowded within .50 p.e. (Urban) (=7.87/3.96) instead of spreading over three or four p.e., as they ought to do were the Urban p.e. reasonably reliable. Of a normal population 26% lies within .50 p.e. of its average; if we take limens at random from an urn, the chance of drawing one from this area is clearly .26. The likelihood that nine successive draws will fall therein is $.26^9$ (row 2); and that all the sub-limens in the 14 cases will cluster so narrowly within the Urban limits is the product of all fourteen numbers in row 2. This equals about 10^{-75}; the probability that the empirical limens would spread no more than they do, were the Urban-values correct, is 1 in 10^{75}. The reader may not be aware how incredible this quantity is. Let us consider it a moment. Of all objects in the visible universe, the nebula Andromeda is said to be of the most remote (*4*, 27), whence light travels one million years before it reaches the eye of a terrestrial astronomer; and yet it is 'only' 10^{24} centimeters away. The whole known universe could be housed within a cube containing 10^{75} cc. That's how 'probable' Urban's p.e. are.

(*2*) Approaching the matter again, we divide the Urban p.e. by the empirical p.e. (Md) of the partial limens, for the 14 cases, and have:

TABLE III

The first figure with each *O* stands for S_1, the second for S_2

	Observers													
	I		II		III		IV		V		VI		VII	
1.	12.0	29.2	11.7	22.1	11.5	22.1	9.4	13.5	11.7	9.5	14.5	12.4	12.4	15.7
2.	15	80	15	44	15	44	9	19	15	9	23	15	15	28

In row 1 (*O* I) we find 12.0; that is, Urban's p.e. (3.933) is 12.0 times the empirical p.e. (Md), .328. What is the likelihood that even one limen may be found, upon repeated testing, which will depart from the average by as much as 3.933 (12 p.e.)?[5] From Pearson's table (*5*, 11 and xxi) a value which deviates 12 p.e. (some 8 s.d.) from its average will occur about once in 10^{15} trials; with I, S_2 the chances reduce to 1 in 10^{80}. Row 2 contains the successive exponents of 10. The probability of the whole system of deviations [that even a single limen for each *O* will depart one p.e. (Urban) from the average] is the product of these fourteen quantities, or 1 in 10^{346}. How big this number is I have no idea; but in view of the evidence Urban's figures might well be called the 'im-probable errors.'

(B) Urban's p.e. changes in magnitude as we alter the arbitrary zero or point of reference. This may be tested by the reader if he cares to do so. The complete formula is found in *7*, xvi, 226, with a correction given in *9*, 253, footnote; or better yet, in Thomson, *6*, 55. Let the reader calculate a p.e. using, as Urban did, the regular value of the stimulus-weights (84, 88, ... ,108 grm.) and again with origin referred to 80, so that the stimuli become 4, 8, ... , 28. Since the actual steps remain the same (4 grm.), the change will have no effect whatever upon the actual dispersion;[6] but it

[4]For *O*s I to III, nine S_1 and nine S_2; for *O*s IV to VII, six S_1 and six S_2.

[5]As before, one-half of all future limens ought to deviate from the average by this amount were the Urban-figures true; we merely ask whether a single *one* (out of nine or six) will do so. It is clear, furthermore, that the total limens will scatter less than do the partials; by the common theory of measures, sub-limens ought to spread $\sqrt{9}$ or $\sqrt{6}$ times as far as do the whole limens, S_1 or S_2. The p.e. (Urban), as computed for a sampling case, proved to be 6.90 for *O* I, limen S_1, sub-limen I 2 a (*7*, xv, 268), the figure for S_1 itself being 3.93. We are extremely generous therefore to Urban in applying his p.e. for S_1 and S_2 to the partial limens.

[6]The absolute magnitude of S_0, S_1 and S_2 will of course be reduced by 80 grm., but the distribution itself will be unaffected.

makes a big difference in the Urban p.e. By merely shifting the origin Thomson was able to prove that Urban's p.e. for *O* I, S_2 could be reduced from 9.477 to .39; whereupon he concludes; "Professor Urban's formula can be made to give us any value from this [0.39] upward by merely moving the origin, a procedure which ought to have no effect on the probable error, and has indeed no effect on the corrected formulae given in this paper" (*6*, 55). We see that Urban's formula is not even consistently incorrect; it yields no one determinate value at all for the probable error; what you happen to get depends upon the origin you happen to choose!

(C) Dr. Urban writes: "My presentation of the method of constant stimuli is in agreement with the method of least squares. . . . There exists just one way to refute my formulae: it must be shown that my deduction is in contradiction with one of the rules of least squares. None of my reviewers has done this. . . . At present I no longer hope that an error will be found in my determination of the probable error, which is rather a pity" (*9*, 256).

Professor Thomson long ago made known two errors in Urban's procedure; the more important of these we may briefly examine here. In Urban's method, two values, c (=ah) and h, are first derived; the quotient c/h then yields the limen, a. Knowing the probable errors of c and of h, Urban proceeds to deduce (*7*, 226) the p.e. of their quotient [the limen a (=c/h)] by means of the customary general formula (involving a Taylor-series) for propagation of error by independently measured quantities whatever their functional relation (for the method, see my earlier article, *2*, 86; or any book on least squares *e.g.* Merriman, *3*, 78). This procedure is perfectly sound when we are dealing with *wholly independent variables*; but c and h, far from being unrelated magnitudes, vary with a concomitance that is well-nigh complete. Consider Table IV, which arrays the values

TABLE IV

Correlations of h with c
Observers

	I	IIa	III	IV	V	VI	VII	IIb	VIII
S_1	.983	1.00	.983	1.00	.883	1.00	1.00	.996	.997
p.e.	.008		.008		.052			.001	.001
S_2	1.00	1.00	1.00	1.00	1.00	1.00	.983	.994	.998
p.e.							.010	.002	.001

of ρ (Spearman's rank-correlation) between c and h for Urban's 7 *O*s and two others (*8*, 297-8).[7] It is clear that the functional dependence of c and h is virtually perfect; this is all the more striking in the longer series, IIb and VIII. That h and c (=ah) should change as a rule *pari passu* would seem to be reasonable in theory and here proves to be true in fact. We are told that Urban, in private correspondence with Thomson, "urges that c and h are independent" (*6*, 53). In view of these figures, this claim can no longer be defended, at least not with the above data; and yet, despite their well-nigh perfect correlation, Urban treats c and h as magnitudes which vary in complete independence of each other! If he has been looking for a flaw in his formula, he might do well to renew his quest right here.

A few words may be devoted to sundry comments by Dr. Urban.

(a) "Culler does not do justice to my argument, when he asks how the $\Phi(\gamma)$-hypothesis can be less exact than the arctan-hypothesis. I did not calculate the probable error of the interval of uncertainty by this latter hypothesis," etc. (*9*, 253). The statement is correct. I find that the arctan-procedure is not included among the "vier psychophysischen Methoden" of which the $\Phi(\gamma)$-process is said to be "die ungenaueste" (*7*, xvi, 227). My original question then (*2*, 85) is hardly relevant and is hereby withdrawn.

[7] For I, IIa, III, n = 9; for IV to VII, n = 6; for IIb and VIII, n = 28.

(b) "Culler then compares his results with those obtained by Thomson. This is a mistake if it is meant that these data are different from mine. The point of Thomson's criticism was settled five or six years ago" (*9*, 253). To the last, rather cryptic, remark let me adduce a statement published by Thomson in 1925 (*1*, 74):

"Professor F. M. Urban, some twelve years ago, brought forward reasons which in his opinion showed that the Method of Limits was much more exact than the Constant Method: but his mathematics contained certain errors.[8] The various processes do not differ very widely in this respect if each is used in circumstances favourable to itself, but on the whole the Constant Process is most reliable, and Linear Interpolation least." We see that, as recently as 1925 when the third edition of his book was issued, (1) Thomson still adhered to the criticisms of his 1914 article; (2) he still believed, in categorical opposition to Urban, that "on the whole the Constant Process is most reliable, and Linear Interpolation least." It is clear that, as late as two years ago, Thomson had in no wise modified his opinion about the inaccuracy of Urban's formula nor retracted his original objections; one may be curious to know, therefore, in what sense "the point of Thomson's criticism was settled five or six years ago."

(c) "There is no real difference between his results and mine, provided that one does not stop halfway, as Thomson did" (*9*, 253); "his final results are not much better than mere guesses" (255); "his idea is right, but he did not work it out" (255); "my formulae and those of Thomson are essentially identical" (256). Dr. Urban of course should know whether his formulae are "essentially identical" with Thomson's; but it is clear that Thomson, as late as 1925, did not so consider them, even thought Urban had "communicated his results" (256) to him (presumably five or six years back). Surely anyone may be excused for wondering how Thomson's formula, which yielded .151, where Urban's gave 9.477 (over 62 times as large!), can be essentially identical with his (*6*, 51).

(d) Dr. Urban's book on *Wahrscheinlichkeitsrechnung* did not reach me until a few days ago, since writing this paper. It was not available when I prepared the other article (*2*) and there is no time for me now to read it thoroughly. The book has no special place, however, in our discussion. Dr. Urban is defending and I am attacking the procedure and figures given in the original articles (*7*); there is no need for a 'change of venue.'

To conclude: we have tried to prove that Urban's formula for the probable error of the limen is fatally defective in no less than three ways.

(1) His own figures, when tested by the actual dispersion of the fractional limens, are fantastically improbable.

(2) The formula yields no single, defined value at all. By the simple expedient of shifting the origin, we can get, over an extensive range, any figure whatsoever.

(3) The quantities h and c, whose mutual dependence is virtually perfect, he treats like wholly unrelated variables, as if they were heads and tails in successive throws of a coin.

In closing, I must avow a deep bewilderment over Dr. Urban's paper. I yield to none in admiration of his work in this toilsome and difficult field, and regret his loss to our science. That he, of all people, should seriously defend a procedure whose results are clearly erroneous is almost incredible. Many of these facts have been adumbrated or developed before, notably in Thomson's brilliant critique; but I have searched Dr. Urban's paper in vain for any sign that he is aware of their crushing force. An air of unreality pervades the whole business, which recalls the ancient word of Horace: *bonus dormitat Homerus!*

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[8]Thomson here refers to the article of 1914 (*6*) and to another of 1913.

Of course, it may be I who nods! But the reader has both papers in hand; let him decide who is dozing.[9]

University of Illinois ELMER CULLER

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THE THIRTY-FIFTH ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

The thirty-fifth annual meeting of the American Psychological Association was held at the University of Pennsylvania, December 28-30, 1926. There was a total registration of three hundred and ninety-nine. Two hundred and forty attended the banquet, and a number of persons came in later to hear the presidential address. The final attendance of the evening was three hundred and fifty. There were fourteen ex-presidents of the Association present at the meeting, and members came from thirty-three states and three foreign countries. About one hundred and fifty persons attended each of the main sessions. One hundred and fifty persons were at the first, and sixty at the second experimental round table. The large attendance was entirely unexpected, and the rooms were in consequence frequently over-crowded, but the sessions were started promptly and without confusion, and the local committee deserves sincere praise for its efficient management.

We can expect a considerable increase in the attendance in the future, due to the growth in the membership, and the increasing importance of the annual meeting. Even at the present time, the task of the program committee is not easy. A number of papers had to be rejected, as it is against the policy of the Association to run too many parallel sessions. It is unfortunate for individual members that such elimination must be made, but in the long run the Association will profit by a program of carefully selected papers from a large preliminary list. The program committee worked with great care and foresight, and a well-balanced program was arranged. As usual, there were parallel sessions, which were staggered in order to allow the members to hear a good proportion of the papers. There were never more than two parallel sessions, except on Wednesday afternoon, when three conferences were in session at the same time. I do not include, of course, the corridor meetings which had their usual full attendance. There

[9] The h-method, developed in my former article, has not been treated above. It was originally proposed as a *Notbehelf*, which gave usable values, with full recognition of its approximate character. I have not been satisfied with its present form and shall publish shortly a revised procedure which will make it more accurate and useful.

were six papers in most sessions; a few sessions had seven papers. The latter number makes rather a long program, and leaves little time for discussion, which, even under the best conditions, is seldom very spirited.

As the abstracts will appear shortly in the *Psychological Bulletin*, I shall give only a general account of the papers. Instead of beginning the meeting with the customary session on general psychology, the opening sessions were upon applied psychology and experimental psychology. The session on applied psychology was devoted chiefly to personality studies and vocational problems.

There were ten papers on experimental psychology, distributed over a session and a half. The reports covered a wide range of subjects—the temperature sense, conditioned reflex, memory, sleep, emotions, and learning. The papers on comparative psychology were upon brain localization, auditory sensitivity of the rat, and the factor of age and nutrition in the behavior of the rat.

There were only six papers upon general psychology, in contrast to twelve last year. At the previous meeting, the theory of *Gestalt* was prominent. This year there were no general papers on that subject. A plea was made for instruction in anthropology. There were also papers on instincts, anthropoid research, religion, and equipment for teaching psychology.

The papers on mental measurement dealt with the intelligence of Indians, mental growth, mental organization, and specific problems of intelligence tests. The program for clinical psychology ranged from intelligence tests in the case of brain injury to color preferences of white and negro children.

In the session on abnormal psychology, hypnotism was the subject of several papers. Further work was reported on the perception of words by means of tactual stimuli, and upon certain phases of nerve regeneration. Other topics were compulsions, aphasia, and automatic drawings.

Methods of teaching and the learning process were the chief problems of the sessions on educational psychology. The use of the term attitude in education was criticized. A report was also presented of four tests to determine sex differences.

The session for graduate students was very successful last year, but the one session of fifteen papers was too long. Twenty-one papers were presented this year, but they were divided between two sessions, which was very much of an improvement. These papers have become an important feature of the annual meeting.

Conferences and round tables have grown in popularity. There were three this year, upon psychological research, clinical psychology, and experimental psychology, respectively. The experimental round table extended over two sessions. The first session was so largely attended that it became a general meeting, instead of an occasion for informal discussion.

At the business meeting, it was voted to extend an invitation to the International Congress to meet in America in 1929.

After the banquet, the president, Professor Harvey A. Carr, of the University of Chicago, read an interesting paper entitled 'The Interpretation of the Animal Mind,' which consisted in large part of a critical exposition of Miss Washburn's fundamental concepts of animal behavior.

Princeton University H. S. LANGFELD

'WOLF CHILDREN' OF INDIA

In the autumn of 1926 the writer of this note came across a brief report in one of our daily newspapers to the effect that two children had been discovered in Bengal, India, living in a den with a family of wolves. This report stated that the children had been removed from the wolf-den by the Rev. J. A. L. Singh, head of the orphanage at Midnapur, Bengal, India, and that the children had been taken into this orphanage. Interested in

the account, the writer communicated with the Rev. Singh and received from him a letter dated December 6, 1926, containing a short sketch from the diary that he has kept on the subject of these strange children, together with a short history of his orphanage and various personal references that might be used in case further examination should be desired as to his personal and official status.

The Rev. Singh commenced his work among such natives as Santals, Lodhas, and Kurmis from the time he was head master of the local mission school at Midnapur, this school being affiliated with Calcutta University. Feeling a call for the ministry, Mr. Singh left the education department and in 1912 was ordained to the ministry. From that time onward he came more and more into contact with the natives. In every village that Singh visited he found orphans roaming about in want of food and shelter. Gradually he gathered together a number of orphans and in this manner there evolved Singh's orphanage at Midnapur. This orphanage was started on Singh's own initiative and responsibility and depended upon his personal income. Among those who have taken active interest in the Midnapur orphanage is Mr. W. N. Delivingne, District Judge of Midnapur.

The Rev. Singh informs the writer that he hopes to publish a detailed account of the 'wolf-children.' This projected publication is to include photographs of the children, taken at various intervals after the removal of the children from the den of the wolves to the Midnapur orphanage.

The following paragraphs are quotations from the Rev. Singh's letter referred to above:

"The wolf-children were first seen by natives on various occasions. I heard of these children for the first time on August 26, 1920. The same 'ghost story' was repeated to me on September 24, 1920. The children were seen through a field glass by several people (Europeans and Anglo-Indians), from a distance of about one hundred yards, on the 9th and 10th of October, 1920.

"Three wolves were observed to come out of a tunnel-like passage from their den, closely followed by two cubs; then there appeared a human head covered with bushy hair, with a ghastly look about the face. This head tarried for a little while looking to this side and that side, then a human form came out of the den followed by another human being at its heels. The two children crawled on all fours.

"Excavation of the wolf-den took place on October 17, 1920. I took charge of the wolf-children on November 1, 1920. I brought them home on November 4, 1920. I guessed their ages to be, the elder about eight years and the younger about two years. The younger one died September 21, 1921. She was baptized some time before she expired. We named her Amala. Kamala, the surviving one, was baptized January 1, 1925.

"At the present time Kamala can utter about forty words. She is able to form a few sentences, each sentence containing two, or at the most, three words. She never talks unless spoken to; and when spoken to she may or may not reply. She is obedient to Mrs. Singh and myself only. Kamala is possessed of very acute hearing and evidences an exceedingly acute animal-like sense of smell. She can smell meat at a great distance. She was never known to kill any domestic animal but is fond of pouncing upon any killed animal if found anywhere by her. Never weeps or smiles, but has a 'smiling appearance.' Shed a single tear when Amala died and would not leave the place where she lay dead. She is learning very slowly to imitate. Does not now play at all and does not mingle with other children. Once, both Amala and Kamala somewhat liked the company of an infant by the name of Benjamin while he was crawling and learning to talk. But one day they gave him such a biting and scratching that the infant was frightened and would never approach the wolf-children again. Amala and Kamala liked the company of Mrs. Singh; and Kamala, the surviving one of the pair, is much attached to her. The eyes of the children

possessed a peculiar glare, such as that observed in the eyes of dogs or cats in the dark. Up to the present time Kamala sees better at night than during the daytime and seldom sleeps after midnight. The children used to cry or howl in a peculiar voice, neither animal nor human. Kamala still makes these noises at times. She is averse to all cleanliness, and serves the calls of nature anywhere, wherever she may happen to be at the time. Used to tear her clothes off. Hence, a loin-cloth was stitched to her in such a fashion that she could not open or tear it. Kamala used to eat and drink like a dog, lowering her mouth down to the plate, and never used her hands for the purpose of eating or drinking. She would gnaw a big bone on the ground and would rub it at times in order to separate the meat from the bone. At the present time she uses her hands for eating and walks straight on both legs, but cannot run at all."

University of Vermont PAUL C. SQUIRES

PRELIMINARY REPORT ON A DEMONSTRATION OF EXPERIMENTS ON HYPNOTISM SHOWN BY GUSTAF WALLENIUS, FIL. MAG. (UPSALA)

During the eighth International Congress of Psychology at Groningen' Holland, (Sept. 6-11, 1926), Mr. Wallenius gave a demonstration of experiments, which were a repetition of those performed by the late Professor Alrutz. These experiments were claimed to show that hypnotism is the effect of a 'nervous effluence,' and that it is not, as ordinarily supposed, a psychological function.

At the first sitting (Sept. 9) were present—besides Mr. Wallenius and the subject of the experiment (a Swedish metal-worker)—Professor Janet, Professor McDougall, Professor Roels, Dr. van Loon, Professor Spearman, Dr. Thouless, and (for the last part of the demonstration) Mr. Bartlett. The experiment, which was claimed by Professor Alrutz to be a crucial proof of his theory, was performed under strictly controlled conditions. The next day the same experiment was performed twice more in the presence of Mr. Bartlett, Professor K. H. Bouman, Dr. van Loon, Dr. Myers, Dr. Thouless, and Professor Zwaardemaker.

The methods and the results obtained will be fully reported in a longer communication which we hope to publish in the *Proceedings of the Society for Psychical Research* (London). We can, however, say definitely that no phenomena occurred in this demonstration which could require the theory of nervous effluence.

All those before whom the experiments were demonstrated agreed that the results obtained pointed to the ordinarily accepted theory of suggestion in hypnosis as an adequate explanation of all the phenomena observed.

University of Glasgow F. G. H. VAN LOON
Scotland R. H. THOULESS

THE SEVENTH ANNUAL EDUCATIONAL CONFERENCE

The seventh annual educational conference sponsored by the Ohio State University will be held at that institution on April 7, 8, and 9, 1927. As in former years the general sessions will be held on Thursday evening, Friday evening, and Saturday morning. Men of national note, such as Professor E. L. Thorndike of Teachers College, Columbia University; Professor Wallace W. Atwood of Clark University; Judge Olson of the Juvenile Court of Chicago; Payson Smith, Commissioner of Education for Massachusetts; and others, will address the general sessions.

Both forenoon and afternoon of Friday will be given over to sectional meetings of which this year there will be more than thirty. Here again specialists in their fields will contribute as, for example, Professor S. A. Courtis of Michigan in the Educational and Intelligence Test Section;

Professor S. A. Leonard of Wisconsin in English; Professor Hill of Illinois in Non-biological Science.

The keynote of this year's conference is 'Expertness in Teaching' and the whole program, both general and sectional, will center around this idea. The uniform testimony of the thousands who have attended past conferences is that each year the conference is outstanding for its helpfulness, its abundance of ideas and its lack of mere talk. There is every reason to believe that this year's program equals, if it does not exceed, all former ones.

Ohio State University — E. J. ASHBAUGH

A NEW DEPARTMENT: APPARATUS

A new department, devoted to descriptions of psychological apparatus has, with this issue, been added to the JOURNAL. A change in policy is not involved, as the pages of the JOURNAL have always been open to articles of this kind. Up to the present, however, articles on apparatus have been scattered among different departments of the JOURNAL; sometimes they have appeared among the major and minor articles, at other times among the Notes. Under the present plan these articles will be grouped together. This arrangement, we hope, will be of convenience to our readers, and will receive the support of our contributors.

PSYCHOLOGICAL ABSTRACTS

Since the appearance of the January number, the JOURNAL has received the first three issues of the *Psychological Abstracts*, a monthly magazine published by the American Psychological Association. The new journal publishes abstracts of papers and books, in all languages, on psychology and cognate subjects. The abstracts are non-critical; the contributors do not attempt to evaluate or appraise the articles they review, but merely act in the capacity of reporters.

The journal is edited by W. S. Hunter, of Clark University, with the assistance of R. R. Willoughby, of Clark University, and the coöperation of an international board consisting of F. C. Bartlett, of Cambridge University, England; V. M. Bekhterev, of Leningrad, Russia; Ed. Claparède, of the University of Geneva, Switzerland; G. C. Ferrari, of the University of Bologna, Italy; A. Michotte, of the University of Louvain, Belgium; H. Piéron, of the Sorbonne, Paris, France; M. L. Reymert, of Wittenberg College, Ohio; and W. Wirth, of the University of Leipzig, Germany.

K. M. D.